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**Abstract**[♣]

The availability of a flexible labour force might influence adjustment decisions regarding the rigid part of the labour force. To test this idea, we contrast the use of trainees (fixed-term contracts) and normal-contract workers (open-end contracts) when a reform made it more costly to use trainees. The results of our DID analysis indicate that the burden of adjustment shifts on trainees if they are present in the firm; if this buffer becomes less available, firms employing trainees see their average labour productivity decrease in the short run and their job destruction increase in the medium run. These effects seem to be increasing with firm size.

**JEL:** J23, J31
**Keywords:** fixed term contracts, difference in differences, labour demand, job creation, firing costs.

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## 1. Motivations

This work aims at contributing to the analysis of labour demand determinants and constraints in the Italian labour market. It focuses on firing costs imposed by employment protection legislation and on the role they play in the adjustment process of the labour force at the firm level. To estimate the impact of dismissal regulations on labour demand, we focus on the use of "on the job training" contracts. They are *fixed term contracts* that can be used to hire young workers; their use is supported by the Government, that provides a significant rebate on social security contributions. Being fixed term contracts, they do not generate firing costs when they expire; on the contrary, open-end contracts are expected to generate substantial firing costs when terminated (this point is discussed more extensively below). The availability of a flexible part of the labour force, i.e. that does not generate firing costs, might influence adjustment decisions regarding the rigid part of the labour force (that does generate firing costs) and it might influence overall firm organisation. In particular, we try to answer two questions: "*Are training contracts a buffer to adjust employment?"* and "*What are the effects of a more costly buffer on firm performance?"* The answers are relevant *per se*, but also in a more general sense. In fact, Italian labour market is known to be highly regulated; nevertheless, there is widespread evidence of high flexibility[1]. Hence it becomes important to understand whether institutional constraints are binding on human resources management at the firm level in Italy. This work aims at adding new elements to answer this more general question.

The law regulating "on the job training" contracts changed significantly during our observation period, decreasing the rebate on social security contributions and imposing eligibility rules for firms, thus restricting the availability of the flexible part of the labour force. This provides a sort of natural experiment on the impact this event had on human resource management at the firm level. First of all, we test whether the reform actually had a significant impact on the demand for trainees in our sample. As we get a positive answer on this point, we address the first question (*are training contracts a buffer to adjust employment?*) both providing descriptive evidence on the fact that trainees are more flexible than "normal contract" workers (i.e. workers holding an open-end contract), and testing - using

[1] More details on these points are provided below.

difference in differences analysis - whether trainees and normal contract workers are substitute. We will argue that these two characteristics together identify trainees as a likely buffer to adjust the labour force in response to a shock. A different definition of "buffer" will also be tested, i.e. whether "normal contract" workers are isolated from fluctuations by the availability of a more flexible part of the labour force. Finally, it will be discussed in which sense we can consider "flexible", or "disposable", a group of trainees on which – by definition – the firm is supposed to invest in training.

If the answer to the previous question is positive, as it is the case (i.e. according to our estimates, trainees actually behave as a buffer to adjust employment), then we can go on and estimate the effect of a more costly buffer on firm performance. We may expect average productivity of labour to decrease, because a less flexible labour force is often relatively more distant from its desired level[2]. Or we may want to test if, despite the fact that trainees and normal contract workers are substitute, net job destruction at the firm level is positive, i.e. if total employment decreases because the labour force becomes less flexible. Hence, to answer the second question (*what are the effects of a more costly buffer on firm performance?*), we test – again using difference in differences analysis - whether a more costly buffer is linked to lower job creation and lower average labour productivity. Notice that this is not the same as asking whether an increase in flexibility enhances productivity and job creation; the two effects do not need to be symmetric. This limits the policy interpretation of the following analysis in more contemporary terms, i.e. it cannot easily be extended to the effect of the recent introduction of temporary contracts in the Italian labour market.

In Italy a very small share of the stock of employees works on a fixed term contract; starting from zero in 1984 it reached a maximum of about 8% in 1990; this percentage was 5% in 1996 and it increased again to 7% in 2000[3]. However among them "on the job training" contracts have been the only available fixed term contract till 1993, and in 1996 they still represented about 90% of existing fixed term

[2] If we expect that firms have to overcome non convex adjustment costs to change the level of employment, inaction will emerge in their behaviour, and actual and desired level of employment will possibly be different.

[3] in 1996 inflows (accessions) using "on the job training" contracts represent 10% of total accessions and 15% of accessions of young workers (under 30 years of age); the same figures reached a maximum of 16% and 23% respectively in 1989.

contracts. What matters here is that during our observation period (1990-1992) “on the job training” contracts and fixed term contracts were synonyms.

To motivate the assumption that fixed term contracts do not generate firing costs when they expire and that open-end contracts do, we can refer to several results existing in the literature on adjustment costs. In fact, there is empirical evidence of small, if any, adjustment costs involving temporary contracts, and of substantial costs in adjusting the level of permanent employees in continental Europe. For example, Rota (2001) estimates a labour demand model with fixed and linear adjustment costs using data on open end contracts in medium-sized manufacturing firms located in the north of Italy (close to an upper bound of labour rigidity in Italy). She finds that fixed firing costs are substantial, around 40% of annual wage costs, and that linear firing costs are less important (3.6% of annual wage costs)[4]. Goux et al. (2001) have access to a very rich French dataset where they can actually separate costly and non costly employment changes, i.e. they observe quits, layoffs and retirements as separate events. They estimate - in a convex adjustment cost framework - substantial costs to adjust the level of permanent contract workers; on the contrary they cannot find significant costs involved in adjusting the number of temporary contract workers. Aguirregabiria et al. (1999) - in a linear adjustment cost framework - estimates that firing costs are between one third and a half of the gross annual wage of permanent workers in Spain.

It is worth describing very briefly the Italian institutional framework related to firing costs. In Italy, since the early ‘70s there have been very strong regulations against individual dismissals. On the other hand, collective and negotiated dismissals - because of lack in demand or the need of reorganising production - are supported by public funds to ease the transition of both the firm and the workers to a better condition. Temporary or fixed term contracts have not been available for long time and part time jobs were not easily accessible. Over time and up to these days we went through a slow process of reorganisation of the labour market, toward a more flexible setting. Among other reforms, in 1984 “on the job training” contracts were introduced; over the ‘90s more collective dismissal tools were brought

up; in 1998 temporary contracts and leased work became legal. It must be stressed that medium and large firms were more involved in all this. Very small firms have always been – *de facto* - more able to adjust their labour force through informal channels, despite employment protection legislation. Our data cover both very small and medium sized firms, so we will be able to analyse this aspect from an empirical point of view. In a sense, if we find that institutional constraints are binding in our dataset of small-medium size firms, we can infer that they are very likely to be binding in larger firms as well.

We concentrate on Italy because Italian labour market is an interesting "case study". It is commonly considered a highly regulated market; both employment and wages are supposed to be rigid downward and this downward rigidity disincentives firms to increase employment when the business cycle is positive (Bertola (1992), Bentolila and Bertola (1990)). Contrary to this widely accepted idea, some works present evidence of flexibility higher than expected. OECD Employment Outlook (1994) shows that job turnover in Italy is higher than in Germany and comparable to job turnover in the US. Contini et al (1995) present evidence on substantial worker mobility in Italy (yearly separations involve nearly one third of jobs and one fourth of employees). Del Boca and Rota (1998) present a survey of Italian regulations and of their effects on firms' labour policy. They show that small firms (less than 16 employees) hire and fire workers much more freely than larger firms; that larger firms use *collective* (at least 5 employees) temporary or non temporary layoff tools[5] easily and without administrative impediments. Hence it is worth investigating whether this postulated rigidity is real at the firm level and its potential effects on labour demand. I.e. it is worth addressing the question: "*Are institutional constraints binding on human resources management at the firm level?*" This paper provides a tentative answer.

The paper is organised as follows. Section two briefly presents the data we use; section three discusses the characteristics of training contracts and of their use; section four presents the models we want to estimate to answer the above questions; section five discusses the econometric method applied; section

[4] This is consistent with the importance of collective dismissals and the absence of individual severance payments in the Italian labour market.

[5] Ordinary or special supplementation scheme, mobility scheme.

six answers the questions; section seven concludes. Robustness checks of the results obtained are provided in the appendix.

## 2. The data

A brief foreword about data is necessary, because we use two very different dataset in our analysis (different coverage, different unit of observation, different time span). We use the first - *the employee dataset* - to provide descriptive statistics on the phenomenon as a whole in Italy over a quite long period of time; we use the other - *the firm dataset* - for the econometric analysis as well as for descriptive purposes in the four provinces we study.

Both dataset have been extracted from *Italian Social Security Administration* (INPS) archives. For its institutional purposes, INPS collects data both on employers (firms) and on individual employees. The entire private sector is covered (about 10 million employees and 1.2 million employers per year). Agriculture, self-employment and part of the Public Administration are also covered by INPS, but they are excluded from this study. In administrative archives there are no retrospective questions or pieces of information not related to the specific interest of the Administration that collects the data. On the other hand, the coverage and accuracy of administrative archives cannot be reached by any survey.

### *2.1 Employee data*

Our panel is a random sample from the entire INPS archive of *employees* of private firms. People born on the 10th of March, of June, of September and of December have been selected from the INPS employee archive and observed during the period 1985-1996. The resulting 1:90 random sample has been reorganised into a longitudinal dataset, where the unit of observation is the work-history of the selected employees. There is one record for each employment spell in the year: two forms will be recorded if during year t the worker moves to a new firm. Each worker may be connected at any point in time with his/her firm; it is therefore possible to assign employer's attributes to the employee.

Each record contains (among other things) worker and employer codes, place of work, industry, yearly wage (before tax), number of months, weeks and days for which the wage is paid by the employer, kind of contracts (e.g. full-time, part-time, training contracts) and occupation (defined by 6 broad

categories). Each longitudinal record has been complemented by information on the relevant employer; among others: dates of opening and closure, total number of employees.

### *2.2 Firm data*

Our sample has been drawn from the INPS Archive of private *firms* with employees, and it has been linked to some balance sheet summary statistics[6].

The unit of observation is the firm. Each record contains (among other things) employer code, location, industry, *monthly* number of workers and their wage bill by occupation and contract. It has been linked to some balance sheet summary statistics (e.g. sales, import, export, number of self-employees).

The original dataset includes the *population* of firms in Torino, Parma, Caserta, Taranto. These towns represent different productive areas in Italy. Torino is a large industrial city in the north west of the Country, with rising unemployment rate due to declining manufacturing activities. Parma is a small town in the north east, fast growing area with low unemployment rate. Caserta and Taranto are in the south of Italy, where unemployment rate is high and the economy is weak. It covers the period January 1990 - December 1992: from the peak of a long positive period toward a sharp economic recession.

From the original dataset, we have selected a balanced panel of existing firms (i.e. no entries or exits[7]). This because entry and job creation (or exit and job destruction) are different decisions that in our opinion cannot be described by the same model (see for example Hamermesh, 1993, on this point);

[6] It is courtesy of R&P scrl that provided the matched and anonymous dataset.

[7] Entries and exits are frequent events and involve a substantial part of the labour force, mainly among small firms. About 22% of firms employing less than 6 workers enters or exits the market every year. This figure drops to about 10% among firms employing 6-100 employees. Entries and exits of larger firms are often only apparent and mainly due to mergers, splits, legal transformations. Notice that even if our observation period is not long, the selection of existing firms might cause an over-sampling of "good" firms, because they could survive during a negative phase of the business cycle. However, this seems not to be the case in our sample One way of checking this is to see if selected firms are larger, given their size class, compared to the population. This because we can expect better firms to grow more, on average. We computed (details available on request) that mean employment by size class is only marginally larger in the selected size classes in 1990 (our first year) and that the difference between average size in the selected sample and in the population is decreasing over time, often becoming negative. This indicating that the selection is not strong and, more important, it is not worsening over time when the recession hits.

furthermore, the difference in differences analysis we perform requires data both "before" and "after" the event we are studying, i.e. a balanced panel[8].

Also, we have to exclude firms employing more than 200 employees in January 1990[9] because these are usually multi-plant firms. Each plant is likely to administrate its own workforce; hence pooling plants together - as INPS data do - could hide exactly what we want to study. Furthermore, these firms are usually located in more than one province; if this is the case they are not observed in our dataset based on four provinces only, hence reducing significantly its coverage in this size class. We are aware of the limitations imposed to our analysis by this (necessary) exclusion; notice, however, that on average only about 30% of employees of private firms works in firms above the 200 workers threshold; in fact Italian economy is mainly made of small and medium size firms.

Finally we select manufacturing and private service sectors only[10]. We exclude the construction industry because it includes mainly very small and short-lived firms (every new construction site may appear as a firm in an administrative archive).

After the described selections we have a balanced panel of 25,457 firms; 10,902 of them classified as manufacturing. 1,732 are located in Caserta, 1904 in Taranto, (i.e. 3,636 in the south of Italy), 4,227 in Parma and 17,594 in Torino. They are very small, small and medium size firms, in fact about 60% of our firms employs less than five workers, 10% of them employs more than 20 people.

Comparing the *population*[11] of workers in the four provinces we have and in the macro-areas we claim they represent, we see that - in the population - Torino over-represents North-West of Italy (78% vs. 57% of employees), Parma under-represents North-East (11% vs. 26%) and Caserta and Taranto

[8] We observe the period 1990-1992; the reform we are interested in took place on January the first 1991. Hence we need to observe the firm at least in 1990 and 1991. We analyse also a medium run effect of the reform between 1990 and 1992, hence we need to observe the same firms also in 1992. This produces a balanced sample over the period 1990-1992.

[9] All included firms are kept for the following three years, even if they do not match the size criteria any more. This to avoid endogenous selection.

[10] SIC80 industry codes 2-4 and 6-8.

[11] Population = all private firms with employees; all industries, all size classes, also entries and exits. Source: *Osservatorio INPS*. INPS publishes aggregate statistics on the population of firms and workers they administrate in its *Osservatorio*, on http://www.inps.it/doc/sas_stat/main.html

together under-represent the South (11% vs. 17%); i.e. these four provinces are representative of the economic characteristics of those areas, but their relative weight is different. Our selected sample mirrors quite closely the share of population of employees in the four provinces.

## 3. On the Job Training Contracts

### *3.1 The Law*

"On the job training" contracts are *fixed term contracts* that can be used to hire young workers; their use is supported by the Government, that provides a significant rebate on social security contributions.

More in detail, after 1984 Italian employers can hire young workers (15-29 years old[12]) using this contract. They should provide them with a formal training program; however, the real investment in training the firm does is neither quantified by the law nor exactly checked by the authorities, i.e. there is a positive probability of cheating on this point (we will come back to this below). It is a fixed term contract that can last up to 24 months. It can be transformed into a permanent one when it expires or when it is still valid; in the second case the firm retains the rebate provided by the law till the end of the original training period. Otherwise, the trainee is dismissed at the end of the training contract.

The law reduces significantly the social security contributions due on a given wage (in Italy on average social security contributions are about 50% of the gross wage). The following scheme presents the characteristics of this contract and the evolution over time of the rebate during the period covered by this study[13].

| | year | Firms located in the south or defined artisans | Firms in the trade / tourism industries employing less than 15 workers | All other firms |
|---|---|---|---|---|
| On the job training contract | 1990 | lump sum (98%) | 50 % | 50 % |
| | 1991 - 1992 | lump sum (98%) | 40 % | 25 % |
| Normal contract | 1990 - 1992 | Firms located in the south: full rebate<br>Artisans not in southern regions: no rebate | no rebate | no rebate |

[12] 15-32 years old after June 1991 in some Regions

[13] There can be exceptions, according to what stated by the Regional Commissions for Employment year by year (firms can reach a better or worse category because of the unemployment level in the Region).

It can be seen that Southern firms and artisans[14] enjoy a full rebate all-over the observation period; the other firms enjoy a lower rebate, decreased further by the 1991 reform. This should be contrasted with the characteristics of what we call "normal" contract[15]: an open-end contract that provides no rebate on social security contributions outside the southern regions. At that time in the south of Italy other laws provided full rebate on social security contributions for every employee (incumbent or newly hired, regardless the age[16]).

The saving generated by the use of training contracts is a substantial amount: that provided by social security rebate goes from 12% to almost 50% of yearly wage[17]; that provided by the absence of firing costs can be up to about 40% of the yearly wage[18]; i.e. they sum up to a total of 52% to 90% of the yearly wage. In the north of Italy the incentive to use these contracts is twofold: social security contributions rebate and no firing costs; in the south the incentive is lower: no firing costs. This is likely to generate a different selection of firms using "on the job training" contracts in the two areas; we will come back to this point when presenting descriptive evidence on the use of trainees and again presenting the econometric results.

The law regulating "on the job training" contracts changed significantly during our observation period, decreasing the rebate on social security contributions and imposing eligibility rules for firms, thus restricting the availability of the flexible part of the labour force. In particular, on 1/1/1991 the rebate on trainees' social security contributions decreased significantly for a subset of firms, as the scheme above shows. Furthermore, firms that had not hired on a permanent base at least 50% of their trainees during the previous 24 months were excluded from the use of this contract (no such constraint existed before

[14] Legal definition indicating small manufacturing firms where the owner works in the firm.

[15] As we already said, "on the job training" contracts have been the only available fixed term contract till 1993, i.e. during our observation period (1990-1992) "on the job training" contracts and fixed term contracts were synonyms. "Normal contracts" are open-end ones and generate firing costs when they expire.

[16] These laws changed after 1992.

[17] From 25% to 98% of SSC, when SSC represent almost 50% of yearly gross wage.

[18] Rota (2001). Of course a collective dismissal would generate lower firing costs per capita.

1991). Hence the cost of using this contract increased both in monetary terms, because of the lower rebate, and in terms of rigidity or firing costs, because of the constraint on transformation rate.

In Italy, on aggregate, the effect of the 1991 reform has been massive. Focusing on the dynamic of aggregate employment level of *young* workers by contract (training vs. "normal") over the 1987-1996 period we notice what follows (see Figure 1). In 1988, when there was the first change in the law regulating "on the job training" contracts (they became a bit more expensive) we observe some substitution between normal and training contracts. In 1991 training contracts became much more expensive then before (but still cheaper than normal ones): we observe a massive substitution from training to normal contracts that lasts a couple of years (training contracts can last up to 24 months). Afterwards both groups (training and normal contracts) follow the business cycle.

***Figure 1: absolute changes in the employment level of young workers (under 30): trainees vs. normal contract holders***

Figure 2 focuses on the four provinces we analyse and shows that the stock of trainees decreased in the areas affected by the reform (Torino, Parma) when the reform came into effect (January 1991), and that it was basically constant where the reform did dot change their price (South). In fact in the first two panels of Figure 2 there is a clear break in the series of employees at the end of 1990; then their number decreases smoothly as firms wait for the training contracts to expire to fire them without sustaining any cost. In contrast to this, the stock of normal contract employees, if anything, increased[19] (Figure 3).

***Figure 2: Number of trainees over time***

***Figure 3: Number of "normal" contract workers over time***

The second constraint (firms that had not hired on a permanent base at least 50% of their trainees during the previous 24 months were excluded from the use of this contract) had an obvious impact on the transformation rate (see Table 1). Interestingly, the equilibrium transformation rate, without any constraint, was about 50%; the constraint made it increase by 10 percentage points, but it went back to the previous value in a few years.

19 This is a balanced panel where entries and exits are excluded; the economic downturn is less sharp in this selected sample.

***Table 1: Transformation rate from trainee to normal contract in the same firm (% relative to the stock of trainees)***

On aggregate the effect of the reform on the use of training contracts is clearly to reduce their use. We also want to be sure that the reform had a statistically significant effect on the behaviour of the firms observed in our dataset. This because they are small and medium size firms, and as we have already argued, very small firms have always been *de facto* more flexible than the others, despite legal constraints. We formally test this point after presenting the econometric method in the next section.

Before doing so we present a descriptive analysis of the characteristics of the firms using these contracts.

### *3.2 Who uses training contracts?*

Table 2 shows the number of firms eligible for each discount scheme and in parenthesis the number of firms actually using them (a few exceptions are excluded, because of misclassification). The 50% rebate scheme in 1990 lowers to 40% or 25% after the reform. In our firm dataset the eligible firms are split almost in half between the two new schemes, and of course they carry on the existing 50%-contracts till they expire. The number of firms using training contracts decreases steadily over time, in the group of those affected by the reform. The group of non-affected firms (southern Italy and artisans in the north) behaves quite differently after 1990. Basically nothing changes among southern firms, but the number of artisans actually using training contracts decreases in 1991 and 1992. One reason of this different behaviour might be found in the fact that in those years all firms located in the south enjoyed a full rebate of social security contributions, regardless of the kind of contract used. Hence in that area the comparative advantage of training contracts could be found only in the absence of firing costs at termination, i.e. it was less appealing than in the north of Italy and hence less used. For the same reason users were more selected in the south than in the north and hence they may behave differently. A second reason can be found in the fact that in the north of Italy the only eligible firms for the 98% rebate are “artisans”, i.e. very small manufacturing firms. In Italy, manufacturing follows a long run

declining trend, that is noticeable also in our observation period. We will come back to these points checking the robustness of our results to the exclusion of southern regions from the analysis.

***Table 2: Number of firms eligible and (actually using the contract in December), by class of rebate***

Transitions in the use of training contracts are interesting. There is a general decrease in the use of these contracts over time. In the South (Caserta and Taranto), 80% of firms never uses this contract, with respect to 60% in North East (Parma) and 70% in North West (Torino). This again points to the wider range of supplemented contracts available only in Southern regions. About 10% of firms used these contracts both in 1990 and 1991 in the South; the percentage decreased to 6.4 between 1990 and 1992. The same figures are about 20% and 12% in North East; 11.4% and 8.1% in North West.

Notice that the use of training contracts increases as the provided rebate decreases (Table 3). In fact, the law provides the highest rebate to the "worst" firms, as it is targeted to help small/weak firms to hire young workers. This is why the lowest labour cost is available to (usually small) firms in the Southern regions and the highest one must be paid by (usually medium-large) firms in the Northern regions. However, the latter are likely to use training contracts more extensively on average, because they are "good" firms, i.e. more innovative in their internal organisation, more ready and able to use new instruments, more adaptable to a new environment.

Furthermore, very small firms do not use training contracts very much; their use increases with firm size (Table 3). This is consistent with the observation that very small firms are more likely to be less influenced by employment protection legislation, hence they need to cut firing costs less than larger firms. In fact, for example, gross worker turnover (accessions plus separations over employment stock) of normal contract workers in very small firms is about 50% higher than in medium-large firms.

***Table 3: Percentage of firms actually using training contracts in December, by size and rebate class***

Finally, we performed a simple multivariate analysis to check which firm characteristics are positively or negatively correlated with the use of trainees[20]. Notice that we do not intend to estimate any structural model; we also assume different error structures, as a robustness check[21].

[20] Results available on request.

As expected, the number of trainees is increasing with potential rebate for which the firm is eligible if it decides to use training contracts[22]; it is decreasing in potential wages the firm has to pay to each worker[23]. The number of trainees is positively correlated with the size of the firm, measured by real sales. It increases also with firm's age and the fact that the firm exports or imports some positive amount (proxies for better quality firms, given size). It is positively correlated with the use of other "cheap" contracts, of self-employees and of overtime (i.e. other flexibility enhancing tools). And it is positively correlated with the percentage of white collars (more technologically advanced firms). It is lower in services than in manufacturing. These results are robust to a partition of the dataset into small and medium-large firms.

## 4. The models

### *4.1 A foreword*

Here we present in turns the models we estimate by difference in differences to answer the questions we are interested in. A few preliminary remarks are needed.

First notice that in this paper, to discuss the labour adjustment process we focus on a law whose use is restricted to young workers. Is this choice too limiting in order to understand the overall adjustment dynamic in Italy? The absolute changes in *aggregate* employment level over the last decade give us some insights on the importance of the role young workers play in the adjustment process in this country (see Figure 4). Young people (under 30) bear almost alone the burden of positive and negative changes in total employment.

***Figure 4: Absolute changes in employment level: all workers vs. young workers (under 30)***

[21] First, we estimated a Tobit model on each year in the sample, assuming white noise errors. Then we pooled the observations and estimated a random effect Tobit with the balanced panel obtained. Finally we estimated a logit model with fixed effects, where the dependent variable is just a dummy on the use of the contract in the firm. The drawback of this last method is that only firms that change status (from users to non-users or viceversa) can be used to estimate it.

[22] 50% of the real wage in the cell gives approximately the social security contribution; this is multiplied by the available rebate, e.g. 98%, or 50%, or 40%, or 25%.

[23] The real wage in the cell the firm belongs to, defined by 13 industries, 3 geographical areas, 3 size classes.

Notice also that, given the length of our panel of firms we can answer the questions only from a short run point of view. I.e. we cannot identify a long run movement toward a different equilibrium; we can identify a short term reaction to different prices set by the reform.

Preliminary to all this, we need to test the effect of the 1991 reform on the use of trainees in our dataset, as in previous sections we have only showed its aggregate effect. We might doubt that the reform had any effect on the small and medium size firms that are included in our sample, as we have underlined that this kind of firms may gain flexibility through informal channels. In fact, if we detect a significant impact of this reform on our firms, we can safely infer that it had an effect also on larger firms. Hence, first of all we apply the difference in differences method to test whether the 1991 reform had an effect on the demand of trainees among the firms in our sample.

*4.1.1 Had the 1991 reform an effect on the demand of trainees?*

We have already shown that in Italy on aggregate the effect of the 1991 reform has been a massive substitution of training contracts with normal contracts to hire young workers (Figure 1). We have also shown that in two out of the four provinces we are focusing on (the two involved by the reform) the number of trainees decreased after December 1990 (Figure 2). Here we want to test formally whether the decrease in demand of trainees is statistically significant.

We estimate a very simple demand schedule for trainees at the firm level, as a function of their cost, output and a proxy for the quality of the firm:

$$l_{it} = \gamma^{(l)} D_{it} + X_{it}^{(l)} \beta^{(l)} + u_{it}^{(l)} \qquad (1)$$

where $l$ is the number of trainees, D is a dummy on those affected by the reform of training contracts after the reform took place, X includes a measure of output (real sales) and the share of white collars as a proxy for the level of complexity of technology used by the firm[24]. When the cost of trainees increases

[24] The share of non-production work (white collars / workforce) can proxy the level of complexity of technology and/or of organisation in the firm. Pacelli et al. (1998) show the link between composition of the workforce by occupation and technology in private Italian firms. For example, if we split firms by the intensity of innovation observed in the industry they belong to, we have 43% of white collars in high intensity of innovation sectors and 8% in low intensity of innovation sectors, in the class 1-20 employees; in the class 1000 employees or more these percentages become respectively 51% and 26%.

because of the 1991 reform we expect the firm to decrease its demand for them, i.e. we expect *gamma(l)* to be negative.

### *4.2 Are training contracts a buffer to adjust employment?*

We infer the answer to this question from two different points of view. First, we test the existence of the characteristics of a buffer. To be a buffer, trainees must be more flexible than normal contract workers; they must be hired and dismissed more frequently. And, trainees must be *substitute* of normal contract workers; i.e. if their price increases, demand of normal contract workers should go up. Second, and from a different point of view, we test the existence of the expected effect of a buffer: trainees should be able to isolate normal contract workers from fluctuations of demand, i.e. their presence in the firm should influence not only the number of normal contract workers but also their variability.

To answer the question "are training contracts more flexible than normal ones?" we use some descriptive statistics; the point seems on one side obvious, on the other hand difficult to measure in a convincing way without a structural model that estimates adjustment costs attached to the two kinds of contracts. We address the question "are trainees and normal contract workers substitutes?" estimating a normal contract labour demand equation by difference in differences to single out the effect of the price of trainees on the demand of normal contract workers. And we apply the same method to gross normal-contract worker turnover to test if trainees significantly isolate normal-contract workers from fluctuations.

However, a maybe more basic question needs to be answered: can trainees be used as a buffer by the firm when the firm is supposed to invest in their training? The answer is yes if the investment in training is significantly lower than the saving provided both by social security rebate and by the absence of firing costs (as we said above, from 12% to almost 50% of yearly wage as social security rebate, plus up to 40% of yearly wage as deleted firing costs). Given the high amount of saving provided by this contract, we would not be surprised by the fact that firms invest significantly less than that in training. Furthermore, anedotical evidence points to a large amount of cheating by firms on the training provided to workers hired with this contract. A further indication in this sense can be provided by transformation

rates. Splitting the average transformation rate by occupations or by industries defined by their intensity of innovation we observe a higher transformation rate - as expected - in high tech industries with respect to traditional ones, and among white collars with respect to blue collars. This seems an indication of positive correlation between the degree of skill required by the job and the transformation of the training contract into an open end one; the reason can be a higher investment in specific human capital in these cases. However, the spread in transformation rates is about or below 10 percentage points in both cases (50% vs. 60% both by occupation and by industry in 1990), and both high tech firms and white collars represent a minority of the stock of training contracts (about 5% and 30% respectively). It does not seem likely that the large majority of firms invests heavily in training their trainees.

*4.2.1 Are training contracts more flexible than normal ones?*

As we have already said, it seems that there is some empirical evidence of small, if any, adjustment costs involving temporary contracts, and of substantial costs in adjusting the level of permanent employees in continental Europe. Some descriptive statistics on the relative flexibility of temporary and permanent contract workers in Italy are available; they are not about adjustment costs directly but about observed flows in and out of the firms; these are strictly correlated to the costs involved, even if they are noisy measures.

As training contracts are fixed term obligations, it seems plainly obvious to observe that separation rates are higher for them, even allowing for an average 50% transformation rate at the end of the training period. It may be more convincing to look at accession rates, i.e. total number of accessions (hiring) in year t over the stock of employees at the end of year t[25]. Furthermore, as training contracts are for young workers only while normal contracts are for everybody, we focus on accession rates of young

[25] Notice that using the employee dataset, transformations of training contracts in normal ones are not defined as accessions (this is not true in the firm dataset). Notice also that accessions are not equal to separations if employment is not in steady state, as it is the case here.

workers only. This because job shopping activity at early stages of the working career generates - *ceteris paribus* - a higher number of accessions among young workers[26].

In Table 4 we see that accession rates of trainees are *twice* that of young normal contract workers. We may consider this as preliminary evidence of lower adjustment costs attached to these temporary contracts in Italy.

***Table 4: accession rates, Italy 1986-1996, workers under 30 years of age only***

*4.2.2 Are trainees and normal contract workers substitutes?*

We expect trainees and normal contract workers to be substitutes, but not perfect. This would be consistent with Aguirregabiria et al. (1999), who find that in Spain both level and turnover of permanent contract employees decrease if there are temporary contract workers in the firm, indicating that they are substitutes and that the second could represent a buffer against cyclical fluctuations. However in Spain 15 years after they became legal, the equilibrium share of temporary contracts is far from 100%, indicating non perfect substitutability[27].

We estimate a very simple conditional demand for normal contract workers at the firm level[28]

$$L_{it} = \gamma^{(L)} D_{it} + X_{it}^{(L)} \beta^{(L)} + u_{it}^{(L)} \qquad (2)$$

where L is the number of normal contract workers, D is a dummy on those affected by the reform of training contracts after the reform took place, X(L) includes normal contract workers' real wage, a

[26] A final caveat on this basic statistic is about the fact that the number of trainees is much lower than the number of normal contract workers (600 thousand trainees in 1990 and 2866 thousand young normal contract workers); hence the statistic "accessions over stock" is higher also because of this, given the same absolute value of the numerator. Its effect can be noticed in the very first years in the table, just a few years after the introduction of the law (1984).

[27] Aguirregabiria et al. (1999) explain this non perfect substitutability by the existence of hiring costs also for temporary contracts.

[28] In our labour demand model, capital is kept fixed for simplicity. We focus only on one factor of production, i.e. labour; we analyse the dynamics and interactions among different kinds of labour the firm employs. Of course this is a simplification, justified by the fact that we perform a short run analysis only. However, in the labour productivity model (see below) we will try to control for the investment rate.

measure of output (real sales), the share of white collars as a proxy for technology. When the cost of trainees increases relatively to the cost of normal contract workers we expect the firm to shift toward the use of normal contracts, if they are substitute. I.e. if *gamma(L)* is positive, it means that an increase in trainees' cost of labour (the effect of the reform) triggers a higher demand for normal contract workers: the textbook definition of substitute factors.

As firms have to face turnover costs to adjust the level of normal contract workers, if we estimate a significant positive value of *gamma(L)* we can conclude that the effect of the reform was strong enough to overcome hiring costs and actually change the observed level of employment.

*4.2.3 Are trainees able to isolate normal contract workers from fluctuations of demand?*

Another way of deciding whether trainees are a buffer to adjust employment is to test whether the fact that they are employed by a firm decreases the variability of the number of normal contract workers employed by the same firm. In this case we test the expected effect of a buffer, while in the previous case (are trainees more flexible and substitute of normal contract workers?) we tested the existence of the characteristics of a buffer. The drawback of both proposed methods is that the number of normal contract workers and the level of their turnover desired by the firm as an effect of the 1991 reform may be different from the observed level and turnover. The distance between desired and observed values is a positive function of adjustment costs. If the firm wants to increase the number of normal contract workers – as a reaction to the 1991 reform - it has to face hiring costs; if it wants to increase normal contract worker turnover it has to face hiring and firing costs. It is clear that the first method proposed (that uses the number of normal contract workers) suffers less of this problem compared to the second method (that uses their turnover), as hiring costs are by definition smaller than the sum

of hiring and firing costs. I.e. the reform is more easily effective on the level of employment than on its variability[29].

A more structural model, that includes explicitly hiring and firing costs, is needed to address this point properly; this is done in a companion paper (Pacelli (2002)). Here, we estimate a reduced form model where gross worker turnover of normal contract workers depends on size and quality of the firm as well as on the availability of trainees:

$$GWTL_{it} = \gamma^{(GWTL)} D_{it} + X_{it}^{(GWTL)} \beta^{(GWTL)} + u_{it}^{(GWTL)} \qquad (3)$$

where GWTL is the gross worker turnover of normal contract workers in the firm (sum of monthly accessions and separations from January to December[30], relative to the stock of normal contract workers), D is a dummy on those affected by the reform of training contracts after the reform took place, X(GWTL) includes a measure of output (real sales) and the share of white collars as a proxy for technology. If the reform made trainees less available to isolate normal contract workers from fluctuations, then after the reform took place GWTL should have increased, i.e. we expect *gamma(GWTL)* to be positive.

### *4.3 What are the effects of a more costly buffer on firm performance?*

If the answer to the previous question is positive, i.e. if trainees are a buffer to adjust employment, then we can go on and estimate the effect of a more costly buffer on firm performance. We may suppose that, if it is an adverse condition to be less able to adjust employment easily because the buffer part of the labour force is more costly, than we should be able to measure this effect. For example we may suppose that average productivity of labour decreases, because a less flexible labour force is relatively

[29] There might be an additional problem. Firms may transform a training contract into a normal one; transformation rate is about 50%. Unfortunately we do not observe these events in our firm dataset. Hence, when a training contract is transformed into a permanent one we observe the stock of trainees to decrease by one unit and the stock of normal contracts to increase by one. If the share of trainees in the firm is large enough, this may increase artificially the observed GWT of permanent contract workers.

[30] It excludes December to January employment changes to avoid measurement problems generated by legal transformations of firms. In fact, legal transformations usually take place at the end of the Italian fiscal year (31 December).

more distant from its desired level. Or we may want to test if, despite the fact that trainees and normal contract workers are substitute, net job destruction at the firm level is positive, i.e. if total employment decreases because the labour force becomes less flexible.

Notice that this is not the same as asking whether an increase in flexibility enhances productivity and job creation; the two effects do not need to be symmetric and of the same order of magnitude. This limits the policy interpretation of the following analysis in more contemporary terms, i.e. it cannot easily be extended to the effect of the recent introduction of temporary contracts in the Italian labour market.

To answer these questions, we estimate a productivity equation, where average productivity of labour in the firm is a function of the availability of trainees, controlling for wages, investment and the level of technology

$$\frac{S_{it}}{E_{it}} = \gamma^{(1)} D_{it} + X_{it}^{(1)} \beta^{(1)} + u_{it}^{(1)} \qquad (4)$$

and a total employment equation, as a function of the availability of trainees, controlling for wages, output and the level of technology

$$E_{it} = \gamma^{(2)} D_{it} + X_{it}^{(2)} \beta^{(2)} + u_{it}^{(2)} \qquad (5)$$

We measure the average productivity of labour as average real sales per employee (regardless of the contract). *E* is the sum of trainees and normal contract workers. *X(1)* includes real average wages of employees regardless of the contract, the ratio of durables purchased in the year over sales (a rough proxy for investment rate), the percentage of white collars in the firm (a proxy for technology). *X(2)* includes real average wages of employees regardless of the contract, a measure of output (real sales) and the percentage of white collars in the firm. *D* is again a dummy on the reform after the reform took place.

As the reform made the buffer labour force more costly and hence less readily available, if *gamma(1)* is negative then it means that productivity was depressed by the reform. In the same way, if *gamma(2)* is negative, a less available buffer decreases total employment level, i.e. it depresses job creation.

## 5. Econometric Method

We perform a difference in differences analysis to estimate the coefficient of interest (*gamma*) in equations (1) to (5). The reform involved only a subset of firms; hence it provides a natural control group made of firms that – although they were using training contracts – were not affected by the reform. As we will explain in details below, we can also estimate the common macroeconomic trend, using the same model with data on firms that do not use training contracts

Notice that there is an endogenous selection of firms that decide to use or not to use training contracts; however on 1/1/1991 there was an exogenous[31] selection of users that were involved in the reform and users that were not. We will exploit this exogenous variability in the price of trainees in the difference in differences analysis. We define four groups of firms:

1. those affected by the reform (firms located in the north of Italy not defined artisans) that were employing trainees in 1990 (treatment group g=T)
2. those *non* affected by the reform (firms located in the south and artisans) that were employing trainees in 1990 (control group g=C)
3. those affected by the reform that were *not* employing trainees in 1990 (counterfactual treatment group $g=T_0$)
4. those *non* affected by the reform that were *not* employing trainees in 1990 (counterfactual control group $g=C_0$)

We use two periods: 1990, i.e. before the reform (t=1); 1991 i.e. immediately after the reform (short run effect, t=2). Alternatively we use 1992 to estimate the medium run effect (in this case t=2 is 1992).

Write a generic model

$$y_{it} = \gamma D_{it} + X_{it}\beta + u_{it} \qquad (6)$$

[31] Firms may be endogenously selected into industries or locations or size classes, but this is not due to causes related to the use of training contracts. I.e. no firm switched to a different industry, for example, between 1990 and 1991 to escape the effect of the reform.

where y is a generic dependent variable, X is a matrix of controls and D is the dummy on the reform: D is equal to 1 iif t=2 (after the reform) and g = T. *Gamma* can be estimated using a difference in differences estimator

$$\gamma^{DID} = \left( \overset{\sim}{y}_2^{T} - \overset{\sim}{y}_1^{T} \right) - \left( \overset{\sim}{y}_2^{C} - \overset{\sim}{y}_1^{C} \right) \qquad (7)$$
$$\overset{\sim}{y}_s^{G} = E\left( y_{is} - X_{is} \hat{\beta} \middle| t = s, g = G \right)$$

and exploiting the variability in prices generated by the 1991 reform.

Notice that a proper control for observable and unobservable quality of the firm is crucial to single out the price effect on the quantity of trainees used by the firms. The law provides the highest rebate to the "worst" firms, as it is targeted to help small/weak firms to hire young workers and to invest in training. This is why the lowest labour cost is available to (usually small) firms in the Southern regions and the highest one must be paid by (usually medium-large) firms in the Northern regions. However, the latter are likely to use training contracts more extensively on average, because they are "good" firms, i.e. more innovative in their internal organisation, more ready and able to use new instruments, more adaptable to a new environment. Furthermore these firms are more likely to be able to hire workers, whatever the contract. Between 1990 and 1991 labour cost of trainees increased only for those firms that were already facing the lowest rebate; nothing changed for firms in the south and artisans, that are the most likely to need help to operate. So if we take evidence at its face value we might see that both cost of labour and employment grow in "better" firms. Of course the quality of the firm drives the result.

The error term

$$u_{it} = \psi_i + k_i m_t + \varepsilon_{it} \qquad (8)$$

includes an individual fixed effect, a macro trend that affects individual firms differently and a white noise. We can make two different assumptions about the expected value of the error term, conditional on time, X and the group the firm belongs to. It is standard in this kind of literature (see Blundell et al., 1998) to impose:

$$E\left[ u_{it} \middle| t = s, i \in g, X_{is} \right] = \psi_g + k m_s \qquad (9)$$

the conditional expectation of the individual fixed effect is constant within a group and the macroeconomic environment affects everybody in the same way. Both these assumptions may be problematic in our context. The first because groups are quite heterogeneous and few observables are available to condition on. However, the DID estimator is robust to everything that is constant over time or that grows at a constant rate, even if we do not explicitly control for it. The second hypothesis may be problematic because the affected and non-affected firms are chosen by the policy maker according to the fact that a group is (in some sense) weaker than the other. It might be that groups defined in this way respond differently to the macroeconomic environment. For this reason we relax the second assumption. A looser and maybe more realistic assumption is to impose that the macro-factor can affect groups differently:

$$E\left[u_{it}\middle|t=s, i\in g, X_{is}\right]=\psi_g+k_g m_s \tag{10}$$

Under the more stringent hypothesis on the errors (equation 9), to estimate *gamma* we apply equation (7); in fact

$$E\left[\overset{\sim G}{y}_s\middle|t=s, g=G\right]=y_{is}-X_{is}\beta=\gamma D_{it}+E\left[u_{it}\middle|t=s, i\in g, X_{is}\right]=\gamma D_{it}+\psi_g+km_s \tag{11}$$

$$E\left[\gamma^{DID}\right]=\left[(\gamma+\psi_T+km_2)-(\psi_T+km_1)\right]-\left[(\psi_C+km_2)-(\psi_C+km_1)\right]=\gamma \tag{12}$$

Under the looser hypothesis on the error structure (equation 10) this does not hold. The consistent way of estimating *gamma* in this case is the following. We estimate the same model (equation 1) using the groups $T_0$ and $C_0$. This because, as they choose not to use trainees, they will not show any sign of reaction to the reform. Although users and non-users are self selected we assume that they all face the same macro trend (the policy maker grouped them together). Under equation 10:

$$E\left[\overset{\sim G}{y}_s\middle|t=s, g=G\right]=y_{is}-X_{is}\beta=\gamma D_{it}+E\left[u_{it}\middle|t=s, i\in g, X_{is}\right]=\gamma D_{it}+\psi_g+k_g m_s \tag{13}$$

Using T and C:

$$E\left[\gamma^{DID}\right]=\left[(\gamma+\psi_T+k_T m_2)-(\psi_T+k_T m_1)\right]-\left[(\psi_C+k_C m_2)-(\psi_C+k_C m_1)\right]=\gamma+(k_T-k_C)(m_2-m_1) \tag{14}$$

Using $T_0$ and $C_0$ we estimate the macro trend:

$$E\left[\gamma_0^{DID}\right]=\left[\left(0+\psi_{T_0}+k_{T_0}m_2\right)-\left(\psi_{T_0}+k_{T_0}m_1\right)\right]-\left[\left(\psi_{C_0}+k_{C_0}m_2\right)-\left(\psi_{C_0}+k_{C_0}m_1\right)\right]=\left(k_{T_0}-k_{C_0}\right)\left(m_2-m_1\right) \quad (15)$$

and assuming, as we said, $\left(k_T-k_C\right)=\left(k_{T_0}-k_{C_0}\right)$ (16)

we have $E\left[\gamma^{DID}\right]-E\left[\gamma_0^{DID}\right]=\gamma$ (17)

Finally, we need a consistent estimate of *beta*. To estimate *beta* we pool the two groups and the two periods and we estimate $y_{it}=X_{it}\beta+u'_{it}$ by first differences (as we are including individual fixed effects)[32]. Then we compute

$$\tilde{y}_s^{G}=\sum_{i=1}^{n}\left(y_{is}-X_{is}\hat{\beta}\right)\Big/n \quad for\ i\in G,s \quad (18)$$

and $\hat{\gamma}^{DID}=\left(\tilde{y}_2^{T}-\tilde{y}_1^{T}\right)-\left(\tilde{y}_2^{C}-\tilde{y}_1^{C}\right)$ (19)

As $\tilde{y}_s^{G}$ is a random variable, we use its sample variance to estimate the standard error of $\hat{\gamma}^{DID}$.

For the estimate of *beta* to be consistent, *X* must be uncorrelated to *u'*. But,

$$\begin{aligned} y_{it}&=X_{it}\beta+u'_{it}=X_{it}\beta+(\gamma D_{it}+\psi_i+k_i m_t+\varepsilon_{it})\\ \Delta y_{it}&=\Delta X_{it}\beta+(\gamma\Delta D_{it}+k_i\Delta m_t+\Delta\varepsilon_{it}) \end{aligned} \quad (20)$$

if we estimate the latter by OLS then

$$p\lim\hat{\beta}=\beta+\gamma\frac{\mathrm{cov}(\Delta D_{it}\Delta X_{it})}{\mathrm{var}(\Delta X_{it})}+k_i\frac{\mathrm{cov}(\Delta m_{it}\Delta X_{it})}{\mathrm{var}(\Delta X_{it})}=\beta+B \quad (21)$$

and

$$p\lim\tilde{y}_{it}=y_{it}-X_{it}\left(p\lim\hat{\beta}\right)=y_{it}-X_{it}\beta-X_{it}B \quad (22)$$

noticing that B is constant across groups and over time, as it has been estimated pooling g=T,C and t=1,2 we have

$$\mathrm{plim}\,\hat{\gamma}^{DID}=\left(\tilde{y}_2^{T}-\tilde{y}_1^{T}\right)-\left(\tilde{y}_2^{C}-\tilde{y}_1^{C}\right)=$$

[32] Actually , using IV for the wage if it is included among the controls.

$$= (y_{T2} - X_{T2}\beta - X_{T2}B) - (y_{T1} - X_{T1}\beta - X_{T1}B) - (y_{C2} - X_{C2}\beta - X_{C2}B) + (y_{C1} - X_{C1}\beta - X_{C1}B) =$$

$$= \gamma^{DID} + B[(X_{T2} - X_{C2}) - (X_{T1} - X_{C1})] \quad (23)$$

We still have a consistent estimator of *gamma* if the "distance in term of X" between treatment and control group (the term in square brackets) is constant between t=1 and t=2. This does not seem a too stringent assumption, as Table 5 confirms.

***Table 5: Average value of the coefficient of B***

## 6. Results of the DID analysis

Table 6 shows regressors' means. There is a noticeable difference in size (measured by sales or employment) between firms that use trainees and have been affected by the reform with respect to all other firms. The difference is made of normal contract workers, not of trainees. They also pay higher average wages to their employees, as usual in larger firms. Real wage of trainees is much lower than real wage of normal contract workers in all groups; trainees are younger and have no tenure in the firm, hence they start from the bottom step of the wage scale. Both average sales per employee and the percentage of white collars in the firm are higher among firms that would have been affected by the reform, had they employed trainees.

***Table 6: Relevant variables' means***

In what follows we discuss in turns the estimated values of *gamma* in equations (1) to (5) above. Regressions results are available on request.

### *6.1 Had the 1991 reform an effect on the demand of trainees?*

We estimate the model described by equation (1) by DID. Table 7 shows the estimated values of *gamma*[33]. *Gamma* is negative and significant, confirming that the reform had an effect not only on aggregate in Italy but also on the firms included in our dataset. As a robustness check we selected very small firms (total employment below 5 employees) and small firms (total employment below 16

[33] Regressions available on request.

employees): *gamma*, although smaller, is still negative and significant. This shows that also small firms reacted to the increase in the price of trainees decreasing their use. This is true both including and excluding controls (the level of sales and the percentage of white collars in the firm).

Notice that in this particular case we must assume that the structure of errors is the one described in equation (9), i.e. the more stringent one. In fact to be able to relax this hypothesis and estimate the model assuming that equation (10) holds, we should use firms that do not use trainees to estimate the macro trend, where the dependent variable is zero by definition. We do not believe that the result (*gamma* significantly negative) would change under a different hypothesis on the error term.

***Table 7: Demand of trainees. Difference in differences estimates of gamma***

### *6.2 Are training contracts a buffer to adjust employment?*

#### *6.2.1 Are trainees and normal contract workers substitutes?*

We discussed in section 4 in which sense we can say that trainees are more flexible than normal contract workers. Here we present the results of the DID estimates of the demand of normal contract workers as a function of the price of trainees. Table 8 prints the estimated values of *gamma*[34]. The column labelled *"gamma"* shows the relevant point estimate if we believe in the more stringent hypothesis on the errors (equation 9), the column labelled *"gamma minus macro trend"* is the one if we believe in the looser hypothesis (equation 10).

***Table 8: Demand of normal contract workers. Difference in differences estimates of gamma***

In the short run *gamma* is significant and positive among users, under both assumptions on the error structure. This provides evidence that trainees and normal contract workers are substitute, at least in the short run. The medium run picture is less clear cut; only under the most stringent hypothesis on the error structure *gamma* is positive and significant. In two years firms may be able to adjust in a more complex way; in fact it is true that training contracts can last up to 24 months; however, very few firms have 1990 contracts still in existence in 1992 (about 100 out of 4000).

[34] IV for wages are: 13 industry, 4 size and 3 area dummies.

The size of *gamma* is interesting. The reform increased labour demand for normal contract workers by one half of a worker. On average the user affected group in 1990 employs 17.7 normal contract workers. The effect, although statistically significant, does not seem large. However, if we compute the implied cross wage elasticity we get about 0.3, that is not negligible[35].

Hence, following this approach, it seems that we can answer in a positive way to our first question: *are trainees a buffer to adjust the labour force?*

Several robustness checks confirm the above results (they are reported in the Appendix). We cut the sample by firm size and by industry, we tried different estimation methods for equation 2 and we excluded the south of Italy from the sample. The results by firm size are particularly interesting (Table 9). As we said, very small firms are *de facto* more flexible than larger ones; personal informal relations more than employment protection regulations rule the employer-employee relationship. We do not expect any strong effect of the reform in very small firms, and this is exactly what we obtain in our robustness check: the estimated *gamma* is significantly much lower than in the benchmark case (.149, s.e. .043 instead of .502, s.e. .062), but still negative and significant. Furthermore, when we exclude very small firms (less than 5 employees), we obtain slightly stronger results, i.e. *gamma* is larger among firms employing 5 or more workers (.599, s.e. .096). We also tried a different threshold, 15 employees, because most of employment protection legislation applies only above that threshold. Below 15 employees, benchmark results are confirmed, although *gamma* is significantly smaller than the benchmark case (*gamma* is equal to .301, s.e. .060). Unfortunately, no significant result can be obtained above that threshold (although *gamma* is larger, 0.582, s.e. .348); the sample size above that threshold may be too small to provide precise estimates. However, an indication that the effect of the reform is increasing with firm size proceeds from these results together. And a question arises. We estimate that the reform increased labour demand for normal contract workers on average by only one half of a worker. Might *gamma* have been larger, had we included also large firms in our estimates? Unfortunately, this point cannot be explored further, given the structure of our data.

[35] From Table 6, defining w(l) as average wage plus social security contributions (about 50% of the wage) minus the rebate on SSC: $\varepsilon_{L,w_l} = \frac{\Delta L/L}{\Delta w_l/w_l} = \frac{.526/17.77}{2358.5\left[(1+.5-.5*.25)-(1+.5-.5*.5)\right]/2358.5(1+.5-.5*.5)} = .296$

***Table 9: Demand of normal contract workers. Small or large firms excluded***

*6.2.2 Are trainees able to isolate normal contract workers from fluctuations of demand?*

The effect of the reform on normal contract worker turnover[36] is more difficult to assess, as we discussed in section 4. Normal contract worker turnover is sizeable, as Table 10 shows[37], also in medium-large firms. Table 11 presents the results. No controls are included in equation (3), as they are never significant; however the DID estimate is robust to the effect of omitted characteristics that are constant over time or that grow at a constant rate. *Gamma* is positive and (barely) significant in the medium run under the more stringent hypothesis on the error structure (equation (10)). We can read this as an indication that supports – although not strongly - what obtained above (section 6.2.1).

***Table 10: Gross worker turnover of normal contract workers, by firm size***

***Table 11: Gross worker turnover of normal contract workers. Difference in differences estimates of gamma.***

***6.3 What are the effects of a more costly buffer on firm performance?***

Given what we said so far, we can infer that trainees act as a buffer to adjust employment. Hence, we estimate the effect of a more costly buffer on firm performance. In section 4 we said that if it is an adverse condition to be less able to adjust employment easily because the buffer part of the labour force is more costly, than we should be able to measure this effect on productivity and on employment

[36] Defined as 100*(accessions + separations)/E in year t, excluding December to January employment changes to avoid measurement problems generated by legal transformations of firms. In fact, legal transformations usually take place at the end of the Italian fiscal year (31 December).

[37] However, it is zero in extremely small firms, for example employing only one worker that stays all-over the period. In fact, we have to exclude from the estimate of equation (3) 4224 firms whose median employment is 1 person because they have zero turnover all-over the period.

growth in the firm. Table 12 shows the results[38]. In the short run productivity is decreased, but not in the medium-long run. It decreases by 0.0174 billions lire per worker, i.e. about 8,700 euro. The average productivity per worker was 262 million lire in the user affected group in 1990, i.e. 131,000 euro. The effect, although statistically significant, is not large. On the contrary, no significant effect can be estimated on the level of employment in the short run; however in the medium run, when there is no effect on productivity any more, there is a negative and significant effect on the level of employment. About a third of a worker more would have been employed after the reform in the treated group had the reform not taken place. Again, the effect is significant but small (average total employment in this group of firms is about 20 workers). We can rationalise these results as follows. When the shock hits then productivity reacts (and substitution between trainees and normal contract workers takes place); then as time goes by, the stock of total employment is adjusted and productivity goes back to its previous level. I.e., it seems that firms react to shocks adjusting organisation and output on intensive margins in the short run, and more rigid stocks on extensive margins later on.

***Table 12: Productivity and employment equations. Difference in difference estimates of gamma***

We performed several robustness checks on the two estimated equations, to verify to which extent this conclusion can be sustained (they are presented and discussed in the Appendix). Again, we cut the sample by geographical area, by firm size and we try different estimation methods. In addition, for the productivity equation we try different definitions of total employment, that may be relevant in very small firms (they are a large part of our sample). In general our results are confirmed, although in some cases they are statistically a bit weaker.

Estimating the productivity equation in the short run with small firms only (below both the 4 and the 15 employees thresholds) we obtain no significant effects of the reform (Table 13). Excluding firms employing up to 4 employees, *gamma* decreases from -.017 (s.e. .004) to -.023 (s.e. .005); it decreases further to -.040 (s.e. .008) excluding firms employing less than 15 employees. This is true under the more stringent hypothesis on the error structure (equation 9); unfortunately nothing is significant under

[38] In the productivity equation instrument for the wage is the average wage in the cell defined by size, industry, province. In the employment equation instruments for the wage are 13 industry 4 size and 3 area dummies.

the looser hypothesis (equation 10). This is consistent with the results obtained on equation 2: the effect of the reform seems to be increasing with firm size. The same can be said about the employment equation in the medium run (Table 14). Excluding firms employing up to 4 employees *gamma* decreases from -.281 (s.e. ..092) to -.386 (s.e. .132); it decreases further to -.804 (s.e. .389) excluding firms employing less than 15 employees. Again, this is true under the more stringent hypothesis on the error structure only (equation 9)[39]. When we include only small firms the effect turns positive.

Hence, the same question that arose about substitution effect arises here. Might *gamma* have been larger (in absolute value), had we included also large firms in our estimates? As we said, this point cannot be explored further, given the structure of our data.

***Table 13: Productivity equation (6). Small or large firms excluded***

***Table 14: Employment equation (7). Small or large firms excluded***

## 7. Conclusions

The interest in the effects of firing costs imposed by employment protection legislation on firm organisation and firm performance motivated this work. We focused on the use of "on the job training" contracts and contrasted trainees and "normal contract workers". The availability of a more flexible part of the labour force might influence adjustment decisions regarding the more rigid part of the labour force, and this in turn might influence overall firm organisation and performance.

We exploited the effect that the 1991 reform of this contract had on trainees' use: training contract is a fixed term contract that provides a huge rebate on social security contributions of young workers. In 1991 the rebate decreased for a subset of firms and the use of the contract became a bit more difficult. The aggregate effect of the reform was a massive substitution of young normal contract workers to trainees. The number of firms using this contract and the total number of trainees decreased sharply. A multivariate analysis with firm data confirms that the use of trainees is positively correlated to the

[39] Under equation (10) the decrease of *gamma* in weaker and *gamma* is not significant when including only firms above the 15 employees threshold.

potential rebate provided by the law. Finally, a DID analysis generates a significant and negative effect of the reform on the demand of trainees among the firms included in our dataset.

We wanted to answer two questions: "*Are training contracts a buffer to adjust employment?*" and "*What are the effects of a more costly buffer on firm performance?*"

As we said, we infer the answer to the first question from two different points of view. First, we test the existence of the characteristics of a buffer. To be a buffer, trainees must be more flexible than normal contract workers; they must be hired and dismissed more frequently. And, trainees must be *substitute* of normal contract workers; i.e. if their price increases, demand of normal contract workers should go up. Second, and from a different point of view, we test the existence of the expected effect of a buffer: trainees should be able to isolate normal contract workers from fluctuations of demand, i.e. their presence in the firm should influence not only the number of normal contract workers but also their variability. "*Are training contracts more flexible than normal ones?"* There is some empirical evidence of small, if any, adjustment costs involving temporary contracts, and of substantial costs in adjusting the level of permanent employees in continental Europe. Some descriptive statistics on the relative flexibility of temporary and permanent contract workers confirm the result in Italy. "*Are trainees and normal contract workers substitute?"* A difference in differences analysis provided a positive answer. We estimated a labour demand model of normal contract workers, as a function also of the price of trainees. The cross price effect is positive, statistically significant but quite small. The effect is increasing with firm size. Several robustness checks confirm the validity of these conclusions. "*Can the presence of trainees isolate normal contract workers from fluctuations of demand?"* The answer we get from the data is positive, although not very robust.

We answered the second main question (*What are the effects of a more costly buffer on firm performance?*) testing whether average productivity of labour is decreased by the reform, and whether net job destruction at the firm level is increased by the reform, i.e. if total employment level is lower in firms that employ a less flexible labour force. A difference in differences analysis shows that in the short run productivity is decreased, but not in the medium-long run. The effect is statistically significant, but not large. On the contrary, no significant effect can be estimated on the level of employment in the short run; however in the medium run, when there is no effect on productivity any

more, there is a negative and significant (but again quite small) effect on the level of total employment. Again these effects are increasing (in absolute value) with firm size. It seems that firms react to shocks adjusting organisation and output on intensive margins in the short run, and more rigid stocks on extensive margins later on. This is consistent with the observation that employment – regulated by open-end contracts - is a rigid factor of production. Several robustness checks confirm the validity of these conclusions, although exceptions can be found.

Finally, we can provide a tentative answer to the more general question we asked to motivate this work: *"Are institutional constraints binding on human resources management at the firm level?*" They seem to have a significant effect on labour demand and labour productivity, although these effects are small on average. The effects seem to be increasing with firm size, indicating that employment protection legislation matters more the larger the firm, although a small but statistically significant effect can often be detected in small firms as well. This helps also to understand why Italian labour market is overall quite flexible despite the complex set of employment protection rules that are in place: Italian economy is made mainly of small and medium size firms. For example, in 1996, 26% of employees of private firms worked in firms employing less than 10 people, more than 50% in firms employing less than 50 workers, only 17% in firms employing more than 1000 workers.

Some caveats are necessary in order not to draw too strong policy implications from this study. First of all, the sample we use is not representative of the whole Country. It is not a random sample of the Italian population of firms: it excludes entries and exits, it excludes very large firms, it covers only four provinces (although representative of the different economic environment of different areas in Italy). Hence we have to be careful in generalising these results. Second, this is a short-medium run analysis (up to two years after the reform); we cannot say anything about longer run adjustment processes toward a different equilibrium. Third and more important, we are studying a case in which the flexible part of the labour force became *less* available and we obtain some negative and significant effects on firms' performance. More recently in Italy the flexible part of the labour force became *more* available, due to a wider availability of fixed term contracts; it is neither obvious nor clear whether the effects are

symmetric. Hence we can *not* infer that - now that we have more fixed term contracts - productivity has grown in the short run and job creation has grown in the medium run.

## 8. Appendix: Robustness checks

### *8.1 Demand of normal contract workers*

We perform several robustness checks, using different estimates of the labour demand equation (2) and different sample cuts. The cut by firm size was discussed in section 6. Here we present details about the other robustness checks performed.

Particularly reassuring is the cut that excludes firms located in the south of Italy and uses only artisans in the control group. This because - as we pointed out earlier - in the south, incentives to use training contracts are weaker and hence treatment and control groups are more heterogeneous. Table 15 shows that results are even stronger than in the whole sample. Although incentives to use trainees are still stronger for artisans in the North of Italy (made of rebate plus firing costs) compared to the incentive firms located in the South have (firing costs only), the substitution process is stronger in the North. *Gamma* increases from .502 (s.e. .062) to .678 (s.e. .060) when we exclude the southern provinces from the sample. Furthermore, *gamma* becomes significant also in the medium run. The reason may be that firms located in the South that actually use trainees are more selected, more "in favour" of this contract, *ceteris paribus*, because of the lower incentives.

***Table 15: Demand of normal contract workers. South excluded***

Choosing different specifications for the labour demand model (Table 16), *gamma*'s size becomes slightly smaller but results are unchanged: we get positive and significant estimates of *gamma* in the short run; weaker estimates in the medium run.

***Table 16: Demand of normal contract workers. Different estimation methods***

Cutting the sample by industry (Table 17) we notice that trainees and normal contract workers are substitute both in services and in manufacturing firms. This holds also disaggregating further the two macro-industries, but the results weaken a lot; the IV regressions referred to more disaggregated industries are quite weak as well. This is likely due to the much smaller sample size of these sub-

samples. Finally trade is the only industry that experiences more variability in rebate after the reform. In fact only firms in the trade and tourism industry employing less than 15 workers are eligible for the 40% rebate. The estimates of *gamma* are weakly significant only in the "50% to 25%" rebate group. No significant effect can be estimated in the "50% to 40%" rebate group (quite a small group) [40].

***Table 17: Demand of normal contract workers. By industry***

### *8.2 Productivity equation*

Results by firm size were discussed in section 6. Other robustness checks on the productivity equation (4) show what follows. When we exclude the South of Italy from the control group (Table 18), in the short run results are unchanged (*gamma* is a bit smaller). *Gamma* becomes positive and significant in the medium run (it was positive but insignificant in the benchmark case). We will see in Table 21 that excluding the South of Italy from the employment equation we get positive and significant estimates of *gamma* in the short run, and non significant estimates of *gamma* in the medium run (the benchmark case indicated a negative and significant effect in the medium run only). We also saw in Table 15 that substitution between trainees and normal contract workers is stronger in Northern regions. This seems to generate a positive net effect on total employment in Northern regions and a negative effect on average productivity in the short run (like an over-reaction). The medium run positive effect on average productivity takes place when substitution between the two groups of workers is still significant in northern regions but there is no effect on net job creation any more. Medium run adjustment processes to the shock generated by the reform of training contracts may go at a different speed in different areas of the Country[41]. Unfortunately, we cannot investigate this hypothesis further, as our firm dataset covers only two years after the reform.

***Table 18: Productivity equation. South excluded***

[40] Results available on request.

[41] Furthermore, the control group in this case is made of artisans only, i.e. of manufacturing firms only; there might be an industry effect as well.

Assuming different estimation strategies results are much weaker (Table 19); however we strongly argue against these alternative estimation methods.

***Table 19: Productivity equation. Different estimation methods***

We might want to check whether the definition of total labour force used in the denominator of the dependent variable could influence our results. The changes we are proposing are relevant only in very small firms: for example, if a firm produced the observed amount of sales employing three workers from January to November and only two workers in December, when we use total employment in December in the denominator of the dependent variable we badly overestimate average productivity. Of course the effect would be much milder if the firm employed 180 workers over the year and 175 in December. As our sample is largely made of very small firms, it is worth doing this robustness check.

If we use average employment in the year, instead of the stock at December, the result we get is unchanged with respect to the benchmark case (Table 20). Furthermore, adding the owner of the firm to the workforce would acknowledge the fact that a self-employed person works in artisan firms as a common employee. Our results show that the point estimate of *gamma*, although a bit reduced in absolute value, is still negative and significant[42].

***Table 20: Productivity equation. Different definitions of employment***

### *8.3 Employment equation*

Results by firm size were discussed in section 6. Other robustness checks on the employment equation (5) show what follows. We already said that excluding the South of Italy from the employment equation we get positive and significant estimates of *gamma* in the short run, and non significant estimates of *gamma* in the medium run. In addition to the above comments, we can notice that, as the substitution

[42] We might want to use total employment including also self-employees, although they may contribute differently tro the production process with respect to employees. However, again this would make a difference in very small firms only (self employees are never a large group) and very small firms seldom use self-employment. In fact, median firm size is 7 employees among firms that employ also self-employees, 3 employees among firms that do not employ self-employees; 90$^{th}$ percentile is 35 and 12 employees respectively.

process between trainees and normal contract workers is stronger in northern regions, it may cause net job creation in the short run if trainees cannot be laid off soon enough (they can be laid off at no cost only at the end of their temporary contract).

***Table 21: Employment equation. South excluded***

Choosing different estimation methods we still get a negative and significant effect in the medium run only, although a bit weaker (Table 22).

***Table 22: Employment equation. Different estimation methods***

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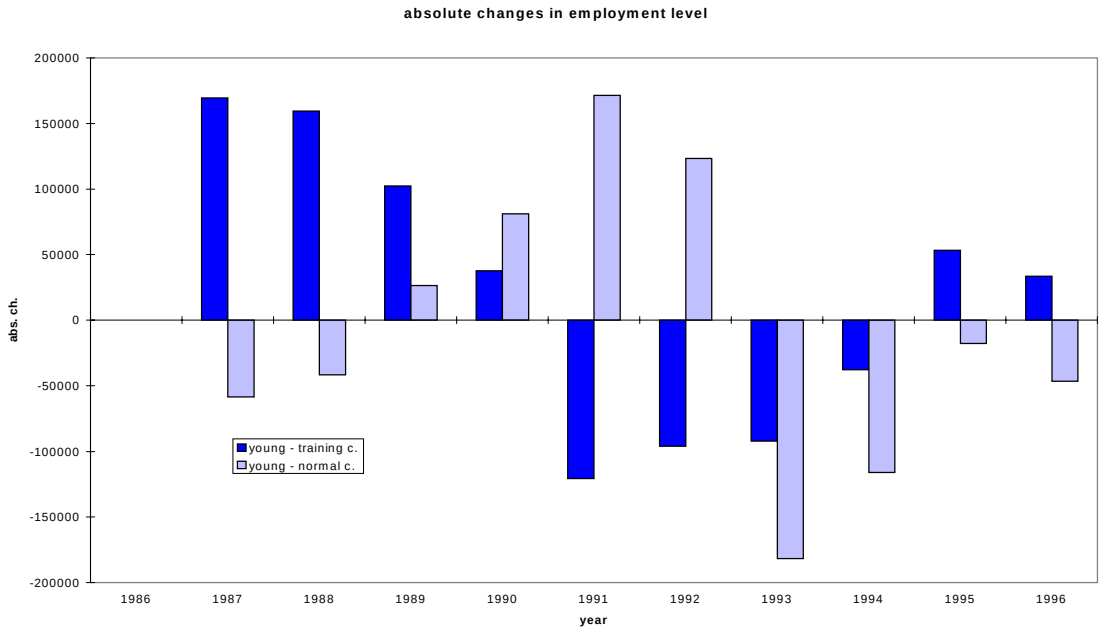
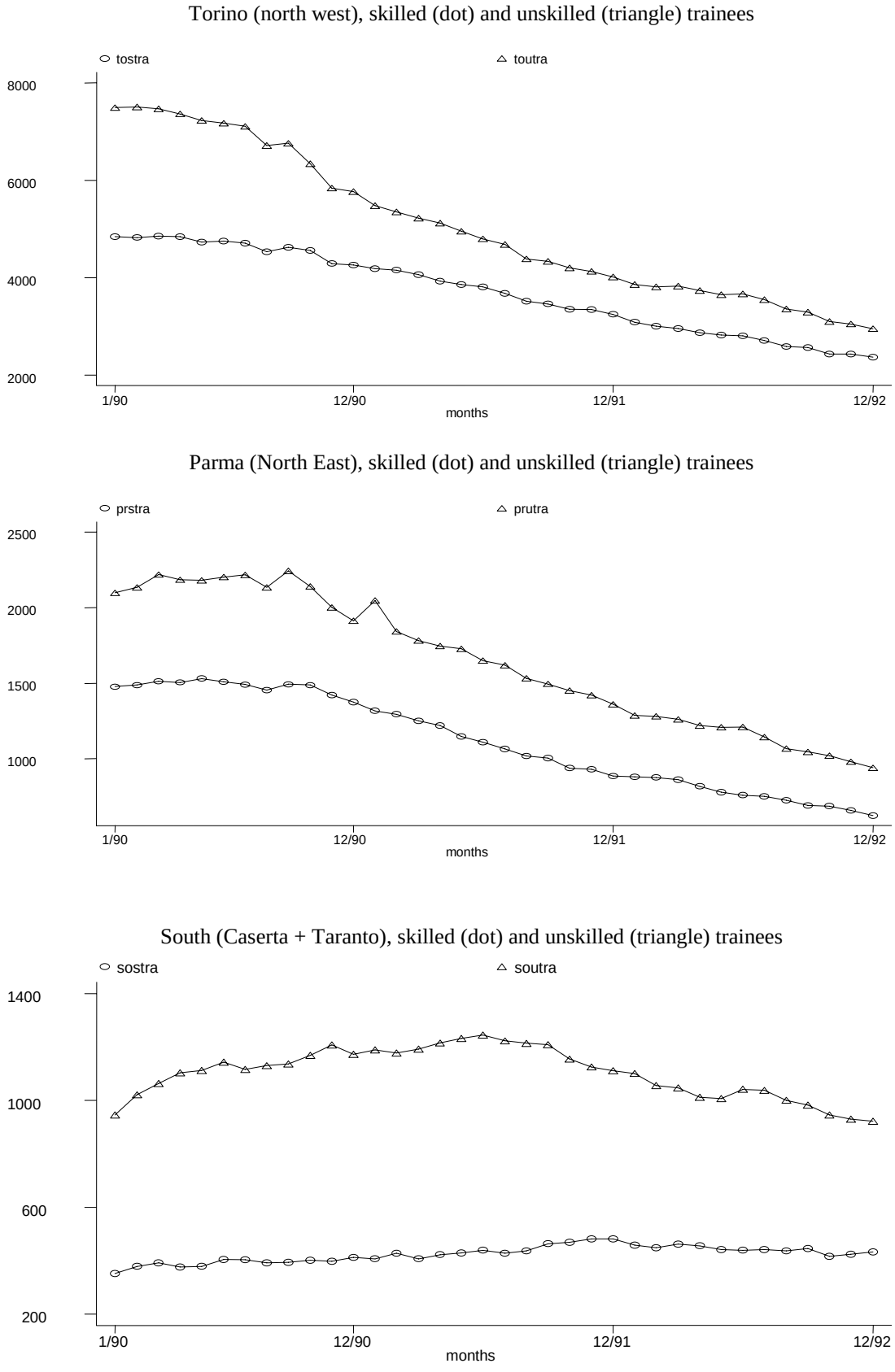
## 10. Figures

***Figure 1: absolute changes in the employment level of young workers (under 30): trainees vs. normal contract holders***



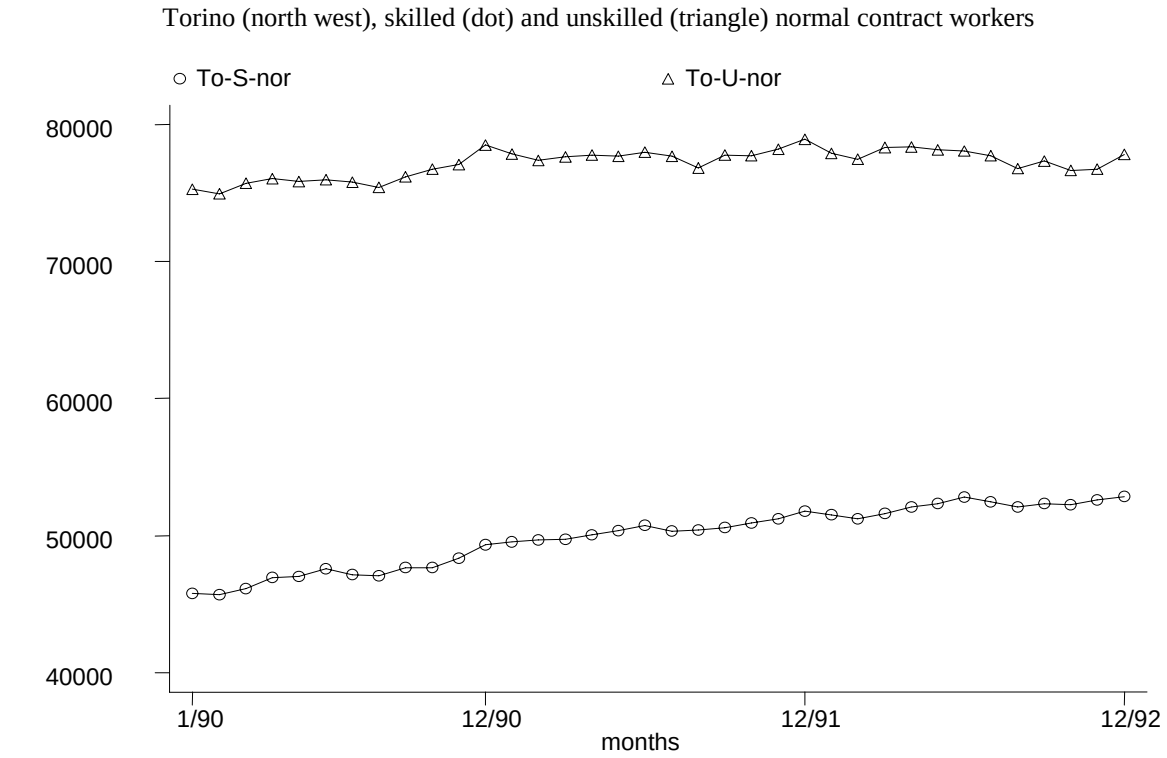

Source: employee data.

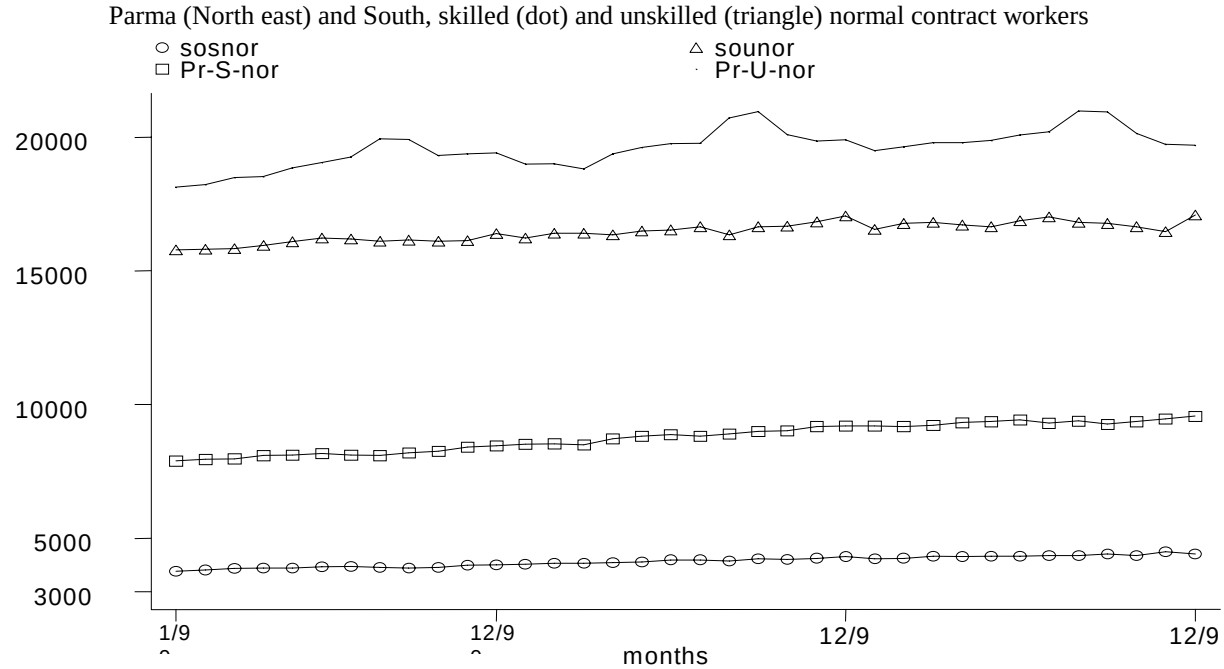
Figure 2: Number of trainees over time




Source: firm data

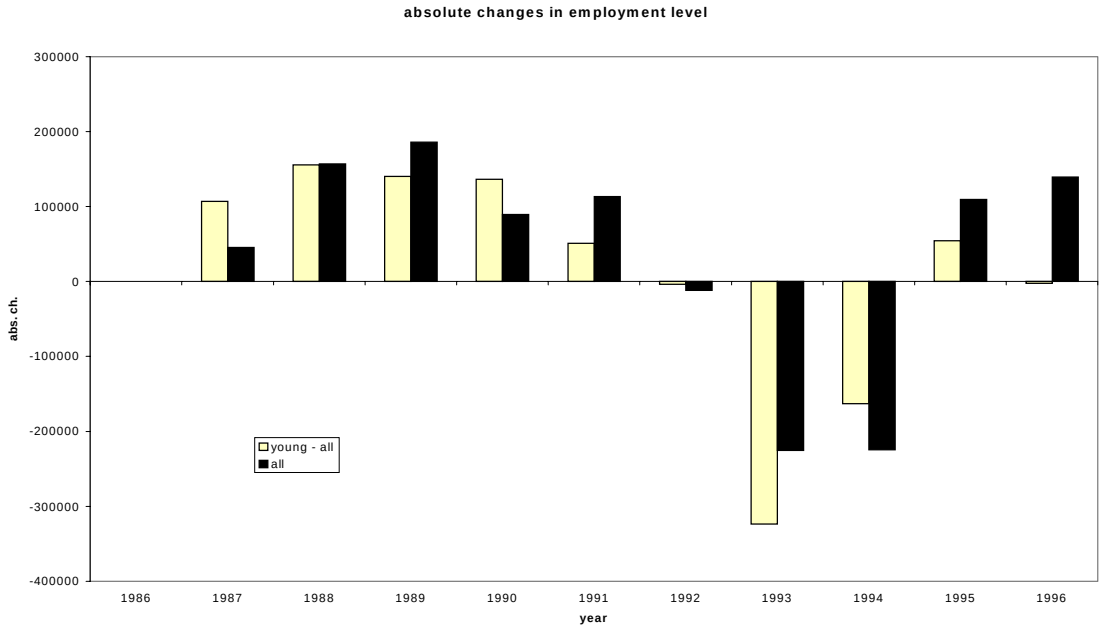
***Figure 3: Number of "normal" contract workers over time***




Source: firm data

***Figure 4: Absolute changes in employment level: all workers vs. young workers (under 30)***




Source: employee data

## 11. Tables

Table 1: Transformation rate from trainee to normal contract in the same firm (% relative to the stock of trainees)

| **Year** | **1986** | **1987** | **1988** | **1989** | **1990** | **1991** | **1992** | **1993** | **1994** | **1995** | **1996** |
|---|---|---|---|---|---|---|---|---|---|---|---|
| **rate** | 45.5 | 45.4 | 49.4 | 52.7 | 51.0 | 61.3 | 60.0 | 63.1 | 61.8 | 55.7 | 45.9 |

Source: employee data

Table 2: Number of firms eligible and (actually using the contract in December), by class of rebate

| | Caserta (South) | Parma (North East) | Taranto (South) | Torino (North West) | Total | % users |
|---|---|---|---|---|---|---|
| Rebate | | | | | | |
| **1990** | | | | | | |
| 98% | 1732 (206) | 1716 (553) | 1904 (301) | 5657 (900) | 11009 (1960) | 17.8% |
| 50% | 0 | 2511 (833) | 0 | 11937 (3133) | 14448 (3966) | 27.4% |
| Total | 1732 | 4227 | 1904 | 17594 | 25457 (5926) | 23.3% |
| **1991** | | | | | | |
| 98% | 1732 (233) | 1716 (485) | 1904 (290) | 5657 (661) | 11009 (1669) | 15.2% |
| 50% | n.a. | n.a. (110) | n.a. | n.a. (116) | n.a. (226) | |
| 40% | n.a. | 1318 (62) | n.a. | 6055 (364) | 7373 (426) | 5.8% |
| 25% | n.a. | 1193 (256) | n.a. | 5882 (1306) | 7075 (1562) | 21.6% |
| Total | 1732 | 4227 | 1904 | 17594 | 25457 (3883) | 15.2% |
| **1992** | | | | | | |
| 98% | 1732 (198) | 1716 (371) | 1904 (303) | 5657 (595) | 11009 (1467) | 13.3% |
| 50% | n.a. | n.a. (3) | n.a. | n.a. (17) | n.a. (20) | |
| 40% | n.a. | 1310 (124) | n.a. | 6032 (397) | 7342 (521) | 7.1% |
| 25% | n.a. | 1201 (279) | n.a. | 5905 (1345) | 7106 (1624) | 22.8% |
| Total | 1732 | 4227 | 1904 | 17594 | 25457 (3632) | 14.3% |

Source: firm data

Table 3: Percentage of firms actually using training contracts in December, by size and rebate class

| **Total Employment at Dec.** | **1990** | | | **1991** | | | | **1992** | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| **rebate class** | **98%** | **50%** | **Total** | **98%** | **40%** | **25%** | **Total** | **98%** | **40%** | **25%** | **Total** |
| E<6 | 10.0 | 13.6 | 11.9 | 8.9 | 7.0 | 12.4 | 8.9 | 7.7 | 5.7 | 8.7 | 7.2 |
| 6>=E<10 | 34.2 | 39.0 | 36.7 | 28.4 | 29.3 | 28.1 | 28.5 | 24.1 | 21.8 | 17.5 | 21.7 |
| 10<=E<20 | 48.2 | 52.4 | 50.9 | 37.4 | 39.7 | 38.2 | 38.1 | 37.4 | 31.3 | 27.7 | 31.9 |
| 20<=E<50[43] | 47.6 | 73.5 | 69.0 | 43.8 | 40.0 | 63.0 | 59.5 | 40.1 | 66.7 | 50.8 | 49.0 |
| 50<=E<100 | 46.4 | 81.4 | 78.6 | 48.4 | - | 74.6 | 72.2 | 58.6 | - | 69.8 | 68.8 |
| 100<=E | 58.3 | 85.4 | 83.5 | 69.2 | - | 78.0 | 77.3 | 61.5 | - | 74.2 | 72.9 |
| Total | 18.2 | 28.5 | 24.0 | 15.4 | 11.9 | 31.5 | 18.9 | 13.7 | 9.2 | 23.9 | 15.3 |

Source: firm data

[43] The 40% rebate is available for firms in the trade and tourism industry employing less than 15 *normal contract workers.* Hence total employment may be above that threshold.

Table 4: accession rates, Italy 1986-1996, workers under 30 years of age only

| year | training contract (l) | normal contract (L) |
|---|---|---|
| 1986 | 1.53 | 0.53 |
| 1987 | 1.12 | 0.45 |
| 1988 | 0.98 | 0.46 |
| 1989 | 0.90 | 0.47 |
| 1990 | 0.81 | 0.48 |
| 1991 | 0.70 | 0.49 |
| 1992 | 0.64 | 0.39 |
| 1993 | 0.64 | 0.37 |
| 1994 | 0.84 | 0.35 |
| 1995 | 0.84 | 0.40 |
| 1996 | 0.74 | 0.39 |

Source: employee dataset.

Table 5: Average value of the coefficient of B

| | year | mean sales (billion lire) | mean % white collars | mean L wage (0,000 lire) | mean E wage (0,000 lire) | mean durables/sales |
|---|---|---|---|---|---|---|
| users affected (T) | 1990 | 5.16 | 0.53 | 3587.25 | 3379.28 | 0.1088 |
| | 1991 | 5.11 | 0.53 | 3640.43 | 3549.61 | 0.0912 |
| | 1992 | 5.78 | 0.54 | 3617.58 | 3558.27 | 0.2022 |
| users non affected (C) | 1990 | 1.46 | 0.19 | 2875.20 | 2721.05 | 0.0671 |
| | 1991 | 1.46 | 0.20 | 2988.66 | 2922.19 | 0.0914 |
| | 1992 | 1.65 | 0.20 | 2941.06 | 2891.23 | 0.0927 |
| | | | | | | |
| Xt1-Xc1 | 1990 | 3.70 | 0.34 | 712.05 | 658.23 | 0.0418 |
| Xt2-Xc2 | 1991 | 3.65 | 0.33 | 651.77 | 627.42 | -0.0001 |
| Xt2-Xc2 | 1992 | 4.13 | 0.34 | 676.52 | 667.04 | 0.1095 |
| | | | | | | |
| (Xt2-Xc2)-(Xt1-Xc1) | 1991 | -0.05 | -0.01 | -60.28 | -30.82 | -0.04 |
| (Xt2-Xc2)-(Xt1-Xc1) | 1992 | 0.43 | 0.00 | -35.53 | 8.81 | 0.07 |
| | | | | | | |
| [(Xt2-Xc2)-(Xt1-Xc1)]/(Xt1-Xc1) | 1991 | -0.01 | -0.03 | -0.08 | -0.05 | -1.00 |
| [(Xt2-Xc2)-(Xt1-Xc1)]/(Xt1-Xc1) | 1992 | 0.12 | 0.00 | -0.05 | 0.01 | -484.46 |

| | year | mean sales (billion lire) | mean % white collars | mean L wage (0,000 lire) | mean E wage (0,000 lire) | mean durables/sales |
|---|---|---|---|---|---|---|
| non users affected (T0) | 1990 | 1.68 | 0.574 | 2964.04 | 2964.04 | 0.1481 |
| | 1991 | 1.71 | 0.577 | 3101.86 | 3080.60 | 0.1246 |
| | 1992 | 1.86 | 0.582 | 3099.43 | 3079.88 | 0.1678 |
| non users non affected (C0) | 1990 | 0.64 | 0.132 | 2558.61 | 2558.61 | 0.0610 |
| | 1991 | 0.64 | 0.136 | 2678.12 | 2664.57 | 0.0775 |
| | 1992 | 0.69 | 0.138 | 2659.21 | 2646.52 | 0.0705 |
| | | | | | | |
| Xt1-Xc1 | 1990 | 1.04 | 0.442 | 405.44 | 405.44 | 0.0871 |
| Xt2-Xc2 | 1991 | 1.07 | 0.442 | 423.74 | 416.03 | 0.0471 |
| Xt2-Xc2 | 1992 | 1.17 | 0.443 | 440.22 | 433.36 | 0.0973 |
| | | | | | | |
| (Xt2-Xc2)-(Xt1-Xc1) | 1991 | 0.03 | -0.001 | 18.30 | 10.59 | -0.04 |
| (Xt2-Xc2)-(Xt1-Xc1) | 1992 | 0.13 | 0.001 | 34.78 | 27.92 | 0.01 |
| | | | | | | |
| [(Xt2-Xc2)-(Xt1-Xc1)]/(Xt1-Xc1) | 1991 | 0.03 | -0.001 | 0.05 | 0.03 | -0.46 |
| [(Xt2-Xc2)-(Xt1-Xc1)]/(Xt1-Xc1) | 1992 | 0.13 | 0.002 | 0.09 | 0.07 | 0.22 |

Table 6: Relevant variables' means

| | Year | No. firms | Total employment | No. normal contract workers | No. trainees | Real average wage of all employees | Real average wage of normal contract workers | Real average wage of trainees | Real sales | Real sales over total employment | % of white collars |
|---|---|---|---|---|---|---|---|---|---|---|---|
| users affected | 1990 | 4120 | 20.44 | 17.77 | 2.67 | 3379.3 | 3587.25 | 2438.5 | 5.16 | 0.262 | 0.53 |
| (T) | 1991 | 4120 | 20.43 | 18.82 | 1.61 | 3549.6 | 3640.43 | 2617.4 | 5.11 | 0.264 | 0.53 |
| | 1992 | 4120 | 20.13 | 19.09 | 1.04 | 3558.3 | 3617.58 | 2546.0 | 5.78 | 0.305 | 0.54 |
| users non affected | 1990 | 2002 | 9.04 | 7.09 | 1.95 | 2721.0 | 2875.20 | 2262.6 | 1.46 | 0.170 | 0.19 |
| (C) | 1991 | 2002 | 9.01 | 7.80 | 1.21 | 2922.2 | 2988.66 | 2478.8 | 1.46 | 0.182 | 0.20 |
| | 1992 | 2002 | 8.86 | 8.06 | 0.80 | 2891.2 | 2941.06 | 2324.6 | 1.65 | 0.209 | 0.20 |
| non users affected | 1990 | 10328 | 5.03 | 5.03 | 0.00 | 2964.0 | 2964.04 | | 1.68 | 0.365 | 0.57 |
| (T0) | 1991 | 10328 | 5.12 | 5.01 | 0.11 | 3080.6 | 3101.86 | 2235.7 | 1.71 | 0.356 | 0.58 |
| | 1992 | 10328 | 5.07 | 4.96 | 0.11 | 3079.9 | 3099.43 | 2341.4 | 1.86 | 0.385 | 0.58 |
| non users non affected | 1990 | 9007 | 4.08 | 4.08 | 0.00 | 2558.6 | 2558.61 | | 0.64 | 0.199 | 0.13 |
| (C0) | 1991 | 9007 | 4.13 | 4.03 | 0.10 | 2664.6 | 2678.12 | 2084.4 | 0.64 | 0.197 | 0.14 |
| | 1992 | 9007 | 4.07 | 3.93 | 0.14 | 2646.5 | 2659.21 | 2243.9 | 0.70 | 0.215 | 0.14 |

Source: firm data
Wages in ten-thousand lire, sales in billion lire

Table 7: Demand of trainees. Difference in differences estimates of gamma

| | ALL | | E<=4 | | E<=15 | |
|---|---|---|---|---|---|---|
| | 1990-1991 | 1990-1992 | 1990-1991 | 1990-1992 | 1990-1991 | 1990-1992 |
| **no controls** | gamma | gamma | gamma | gamma | gamma | gamma |
| gamma | -0.3035 | -0.4686 | -0.1233 | 0.0096 | -0.1140 | -0.0940 |
| s.e. | 0.0296 | 0.0351 | 0.0237 | 0.0261 | 0.0180 | 0.0212 |
| gamma/s.e. | -10.2682 | -13.3481 | -5.1950 | 0.3692 | -6.3208 | -4.4326 |
| | | | X=S,wh% | | X=S,wh% | |
| | 1990-1991 | 1990-1992 | 1990-1991 | 1990-1992 | 1990-1991 | 1990-1992 |
| **X=S,wh%** | gamma | gamma | gamma | gamma | gamma | gamma |
| gamma | -0.3023 | -0.4726 | -0.1217 | 0.0173 | -0.1127 | -0.0961 |
| s.e. | 0.0294 | 0.0352 | 0.0233 | 0.0261 | 0.0179 | 0.0213 |
| gamma/s.e. | -10.2718 | -13.4390 | -5.2207 | 0.6629 | -6.2895 | -4.5105 |

Table 8: Demand of normal contract workers. Difference in differences estimates of gamma

| | 1990-1991 | 1990-1991 | 1990-1992 | 1990-1992 |
|---|---|---|---|---|
| | gamma | gamma - macro trend | gamma | gamma - macro trend |
| gamma | 0.526 | 0.502 | 0.194 | 0.123 |
| s.e. | 0.060 | 0.062 | 0.090 | 0.093 |
| gamma/s.e. | 8.784 | 8.084 | 2.160 | 1.323 |

Source: firm data

Table 9: Demand of normal contract workers. Small or large firms excluded

| | 1990-1991 | 1990-1991 | 1990-1992 | 1990-1992 |
|---|---|---|---|---|
| | Gamma | Gamma - macro trend | gamma | gamma - macro trend |
| E>4 | | | | |
| gamma | 0.592 | 0.599 | 0.113 | 0.115 |
| s.e. | 0.081 | 0.096 | 0.129 | 0.148 |
| gamma/s.e. | 7.279 | 6.207 | 0.880 | 0.775 |
| no. firms | 4221 | | 4218 | |
| E<=4 | | | | |
| gamma | 0.195 | 0.149 | -0.048 | -0.161 |
| s.e. | 0.041 | 0.043 | 0.061 | 0.064 |
| gamma/s.e. | 4.752 | 3.499 | -0.788 | -2.497 |
| no. firms | 1901 | | 1897 | |
| | | | | |
| E>15 | | | | |
| gamma | -0.089 | 0.582 | -1.101 | -0.584 |
| s.e. | 0.190 | 0.348 | 0.391 | 0.571 |
| gamma/s.e. | -0.469 | 1.670 | -2.818 | -1.022 |
| no. firms | 1575 | | 1575 | |
| E<=15 | | | | |
| gamma | 0.374 | 0.301 | 0.049 | -0.093 |
| s.e. | 0.059 | 0.060 | 0.065 | 0.067 |
| gamma/s.e. | 6.371 | 5.021 | 0.754 | -1.380 |
| no. firms | 4547 | | 4540 | |

Table 10: Gross worker turnover of normal contract workers, by firm size

| size | 1990 | 1991 | 1992 |
|---|---|---|---|
| E<6 | 69.9 | 64.0 | 70.0 |
| 6>=E<10 | 63.3 | 54.7 | 56.1 |
| 10<=E<20 | 53.9 | 47.7 | 49.9 |
| 20<=E<50 | 49.0 | 41.9 | 44.2 |
| 50<=E<100 | 38.1 | 33.5 | 40.2 |
| 100<=E | 49.3 | 42.9 | 40.5 |

Source: firm data

Table 11: Gross worker turnover of normal contract workers. Difference in differences estimates of gamma.

| | 1990-1991 | 1990-1991 | 1990-1992 | 1990-1992 |
|---|---|---|---|---|
| | gamma | gamma – macro trend | gamma | gamma - macro trend |
| gamma | -0.025 | 0.021 | -0.022 | 0.069 |
| s.e. | 0.020 | 0.031 | 0.027 | 0.037 |
| gamma/s.e. | -1.263 | 0.677 | -0.811 | 1.864 |

Table 12: Productivity and employment equations. Difference in difference estimates of gamma

| | 1990-1991 | 1990-1991 | 1990-1992 | 1990-1992 |
|---|---|---|---|---|
| | gamma | gamma - macro trend | gamma | gamma - macro trend |
| **S/E** | | | | |
| gamma | -0.0174 | -0.015 | 0.0049 | 0.004 |
| s.e. | 0.0028 | 0.004 | 0.0033 | 0.004 |
| gamma/s.e. | -6.1396 | -3.398 | 1.4989 | 0.857 |
| | | | | |
| **E** | | | | |
| gamma | 0.040 | 0.019 | -0.281 | -0.300 |
| s.e. | 0.056 | 0.059 | 0.092 | 0.095 |
| gamma/s.e. | 0.720 | 0.325 | -3.058 | -3.148 |

Source: firm data

Table 13: Productivity equation (6). Small or large firms excluded

| | 1990-1991 | 1990-1991 | 1990-1992 | 1990-1992 |
|---|---|---|---|---|
| | gamma | Gamma – macro trend | gamma | gamma - macro trend |
| **E>4** | | | | |
| gamma | -0.0237 | 0.0512 | 0.0065 | -0.0108 |
| s.e. | 0.0047 | 0.0611 | 0.0041 | 0.0112 |
| gamma/s.e. | -5.0663 | 0.8378 | 1.5801 | -0.9653 |
| no. firms | 4221 | | 4218 | |
| **E<=4** | | | | |
| gamma | 0.0015 | 0.0044 | -0.0039 | 0.0003 |
| s.e. | 0.0041 | 0.0050 | 0.0054 | 0.0061 |
| gamma/s.e. | 0.3614 | 0.8820 | -0.7332 | 0.0427 |
| no. firms | 1901 | | 1897 | |
| **E>15**[44] | | | | |
| gamma | -0.0398 | -0.0175 | -0.0508 | -0.0327 |
| s.e. | 0.0079 | 0.0399 | 0.0108 | 0.0885 |
| gamma/s.e. | -5.0243 | -0.4370 | -4.7236 | -0.3693 |
| no. firms | 1575 | | 1575 | |
| **E<=15** | | | | |
| gamma | -0.0011 | 0.0017 | 0.0068 | 0.0072 |
| s.e. | 0.0034 | 0.0047 | 0.0037 | 0.0044 |
| gamma/s.e. | -0.3256 | 0.3533 | 1.8501 | 1.6233 |
| no. firms | 4547 | | 4540 | |

Table 14: Employment equation (7). Small or large firms excluded

| | 1990-1991 | 1990-1991 | 1990-1992 | 1990-1992 |
|---|---|---|---|---|
| | Gamma | Gamma - macro trend | gamma | gamma - macro trend |
| **E>4** | | | | |
| gamma | 0.054 | 0.020 | -0.386 | -0.342 |
| s.e. | 0.079 | 0.096 | 0.132 | 0.150 |
| gamma/s.e. | 0.690 | 0.203 | -2.933 | -2.283 |
| no. firms | 4221 | | 4218 | |
| **E<=4** | | | | |
| gamma | 0.029 | -0.002 | 0.134 | 0.070 |
| s.e. | 0.036 | 0.039 | 0.045 | 0.049 |
| gamma/s.e. | 0.787 | -0.043 | 2.963 | 1.426 |
| no. firms | 1901 | | 1897 | |
| **E>15** | | | | |
| gamma | -0.447 | 0.215 | -0.804 | -0.201 |
| s.e. | 0.214 | 0.385 | 0.389 | 0.588 |
| gamma/s.e. | -2.087 | 0.558 | -2.068 | -0.341 |
| no. firms | 1575 | | 1575 | |
| **E<=15** | | | | |
| gamma | 0.131 | 0.067 | 0.127 | 0.044 |
| s.e. | 0.033 | 0.036 | 0.046 | 0.050 |
| gamma/s.e. | 3.936 | 1.868 | 2.786 | 0.888 |
| no. firms | 4547 | | 4540 | |

[44] In this group of regressions the F test on joint significance of coefficients is often unable to reject the null hypothesis. Sample size might be too small. Result should be read as referring to mean values only, not to residuals.

Table 15: Demand of normal contract workers. South excluded

| | 1990-1991 | 1990-1991 | 1990-1992 | 1990-1992 |
|---|---|---|---|---|
| | gamma | gamma - macro trend | gamma | gamma - macro trend |
| gamma | 0.769 | 0.678 | 0.660 | 0.531 |
| s.e. | 0.058 | 0.060 | 0.079 | 0.083 |
| gamma/s.e. | 13.248 | 11.233 | 8.340 | 6.418 |

Table 16: Demand of normal contract workers. Different estimation methods

| | 1990-1991 | 1990-1991 | 1990-1992 | 1990-1992 |
|---|---|---|---|---|
| | gamma | Gamma – macro trend | gamma | gamma - macro trend |
| **no Xb, only mean L** | | | | |
| gamma | 0.3321 | 0.311 | 0.3441 | 0.272 |
| s.e. | 0.0515 | 0.054 | 0.0925 | 0.096 |
| gamma/s.e. | 6.4524 | 5.750 | 3.7187 | 2.840 |
| | | | | |
| **Xb, no IV** | | | | |
| Gamma | 0.3170 | 0.289 | 0.1709 | 0.100 |
| s.e. | 0.0502 | 0.053 | 0.0712 | 0.075 |
| gamma/s.e. | 6.3100 | 5.473 | 2.4007 | 1.333 |
| | | | | |
| **Xb, IV for wage and output** | | | | |
| gamma | 0.467 | 0.430 | -0.156 | -0.343 |
| s.e. | 0.114 | 0.115 | 0.105 | 0.111 |
| gamma/s.e. | 4.109 | 3.730 | -1.492 | -3.084 |

Table 17: Demand of normal contract workers. By industry

| | 1990-1991 | 1990-1991 | 1990-1992 | 1990-1992 |
|---|---|---|---|---|
| | Gamma | Gamma - macro trend | gamma | gamma - macro trend |
| **Manufacturing** | | | | |
| gamma | 0.497 | 0.624 | -0.968 | 1.157 |
| s.e. | 0.080 | 0.091 | 0.153 | 0.206 |
| gamma/s.e. | 6.206 | 6.875 | -6.343 | 5.610 |
| no. firms | 3334 | | 3331 | |
| **Services** | | | | |
| Gamma | 0.373 | 0.385 | -0.231 | -0.328 |
| s.e. | 0.110 | 0.111 | 0.167 | 0.170 |
| gamma/s.e. | 3.391 | 3.453 | -1.378 | -1.935 |
| no. firms | 2788 | | 2784 | |

Table 18: Productivity equation. South excluded

| | 1990-1991 | 1990-1991 | 1990-1992 | 1990-1992 |
|---|---|---|---|---|
| **S/E** | gamma | gamma - macro trend | gamma | gamma – macro trend |
| Gamma | -0.0128 | -0.0088 | 0.0139 | 0.0135 |
| s.e. | 0.0027 | 0.0044 | 0.0029 | 0.0041 |
| Gamma/s.e. | -4.6846 | -2.0181 | 4.7733 | 3.2735 |

Table 19: Productivity equation. Different estimation methods

| | 1990-1991 | 1990-1991 | 1990-1992 | 1990-1992 |
|---|---|---|---|---|
| **S/E** | gamma | gamma – macro trend | gamma | gamma – macro trend |
| **No Xb** | | | | |
| Gamma | -0.0100 | -0.003 | 0.0046 | 0.001 |
| s.e. | 0.0018 | 0.003 | 0.0033 | 0.004 |
| Gamma/s.e. | -5.6946 | -0.924 | 1.3989 | 0.154 |
| | | | | |
| **Xb no IV** | | | | |
| Gamma | -0.0083 | -0.0004 | 0.0052 | 0.002 |
| s.e. | 0.0017 | 0.0029 | 0.0032 | 0.004 |
| Gamma/s.e. | -4.8058 | -0.1302 | 1.6196 | 0.575 |

Table 20: Productivity equation. Different definitions of employment

| | 1990-1991 | 1990-1991 | 1990-1992 | 1990-1992 |
|---|---|---|---|---|
| | gamma | gamma - macro trend | gamma | gamma – macro trend |
| **E+1 (owner)** | | | | |
| gamma | -0.0139 | -0.0125 | 0.0049 | 0.0039 |
| s.e. | 0.0021 | 0.0030 | 0.0022 | 0.0030 |
| gamma/s.e. | -6.5397 | -4.1337 | 2.2008 | 1.3074 |
| **mean E** | | | | |
| gamma | -0.0180 | -0.0142 | 0.0010 | 0.0013 |
| s.e. | 0.0025 | 0.0032 | 0.0030 | 0.0040 |
| gamma/s.e. | -7.0959 | -4.3988 | 0.3405 | 0.3241 |

Table 21: Employment equation. South excluded

| | 1990-1991 | 1990-1991 | 1990-1992 | 1990-1992 |
|---|---|---|---|---|
| **E** | gamma | gamma - macro trend | gamma | gamma - macro trend |
| Gamma | 0.280 | 0.181 | 0.037 | -0.066 |
| s.e. | 0.052 | 0.055 | 0.082 | 0.085 |
| Gamma/s.e. | 5.331 | 3.261 | 0.448 | -0.772 |

Table 22: Employment equation. Different estimation methods

| | 1990-1991 | 1990-1991 | 1990-1992 | 1990-1992 |
|---|---|---|---|---|
| **E** | gamma | gamma – macro trend | gamma | gamma - macro trend |
| **No Xb** | | | | |
| Gamma | 0.029 | 0.001 | -0.125 | -0.172 |
| s.e. | 0.057 | 0.060 | 0.095 | 0.099 |
| Gamma/s.e. | 0.500 | 0.015 | -1.307 | -1.741 |
| | | | | |
| **Xb no IV** | | | | |
| Gamma | 0.018 | -0.014 | -0.281 | -0.323 |
| s.e. | 0.055 | 0.058 | 0.092 | 0.095 |
| Gamma/s.e. | 0.322 | -0.237 | -3.054 | -3.393 |